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WM. W. PAYNE, EDITOR,

Director of Carleton College Observatory.

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THE SIDEREAL MESSENGER,

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DIRECTOR OF CARLETON COLLEGE OBSERVATORY, NORTHFIELD, MINN.

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THE GREAT NEBULA IN ORION.

WILLIAM H. PICKERING.

FOR THE MESSENGER.

Since its earliest discovery this object has occupied a prominent place in the literature of astronomy, but it is only within a few years since the recent applications of photography that an adequate means has been found properly to represent its structure. As this object is now in its most favorable position for observation, I take this opportunity to describe some of our more recent results, hoping that others may confirm them the present year.

We have lately photographed the Trapezium, showing all six stars very clearly. Their unequal magnitudes make this a very good test object, and I think that no one should feel that he is getting the best results if his instrument will not show the sixth star well separated, and the fifth, which is decidedly fainter than the sixth, clearly defined. For this class of work an enlarging apparatus should be attached to the telescope.

We have recently considerably extended the nebulosity about θ by giving longer exposures, and using a quicker lens. The connection with c is now well marked, while the long nebulous streak extending southwards from ζ is broadened and joins c upon the other side, connecting the sword handle with the belt. This extension is of much greater area than the other two nebulæ combined. Its northern portion as far as $3^\circ\ 30'\ S$ is fairly conspicuous, and makes an excellent test object, not of the instrument or the steadiness, but of the clearness of the air, and the blackness of the sky. Owing to the recent advances in stellar photography, this matter of sky illumination has assumed considerable importance, and it is very doubtful if any of the fainter nebulous extensions here described can be photographed at any Observatory lo-

cated in or near a large city. This is due undoubtedly in part to the gas, but chiefly to the electric lights, which illuminate the slight haze found in the sky of nearly all localities with an almost inperceptible light, but which is nevertheless, very destructive to the fainter detail shown by our most sensitive plates when used with long exposures. The difficulty is analagous to the impossibility of photographing the solar corona without an eclipse, or of photographing the zodiacal light during an eclipse.

An interesting structure brought out upon our plates is a large spiral nebula whose outer extremity starts in the vicinity of γ Orionis. It passes about four degrees north of ζ extends to thence to β, then north to η, with an outside stream lying nearly north and south, and preceding β about four degrees. Another stream lying nearly east and west precedes η about the same amount. This nebula is about seventeen degrees in length, by nearly the same breadth, and surrounds a cluster of bright stars including the belt and sword handle. and extending towards γ. The region containing the nebula is noticeably lacking in stars brighter than the eighth magnitude, but contains the very bright stars γ, and β. It is possible that a plate with double our present exposures, which we are soon going to try, will fill the space between η and ζ, thus making the great nebula the inner termination of the spiral. This nebula is shown by three different exposures and is very distinctly marked.

All the above described negatives were taken at our California station upon Wilson's Peak (altitude 6250 feet). Those showing the nebulæ were taken with a Voigtländer portrait lens of 2.6 inches aperture and 8.6 inches equivalent focus, with an exposure of three hours. Stars eleven to the twelfth magnitude are well shown. A more detailed account of these observations with illustrations is in course of preparation, but will probably not appear in time to be of use to observers this year.

Dr. Lewis Swift, director of the Warner Observatory, has discovered another comet. We regret to hear this. The market is overstocked with comets already and there is nothing in our tariff law to prevent their importation and thus bearing the market. How would it do to pension Dr. Swift and put him on the retired list.—*Burlington Hawkeye.*

XUM

ON THE STABILITY OF THE RINGS OF SATURN.

PROFESSOR GEORGE W. COAKLEY.

FOR THE MESSENGER.

It is now nearly three hundred years since Galileo caught the first imperfect view of the rings of Saturn; and more than two hundred years since Huyghens made out their complete character as thin broad circular rings surrounding the planet, but detached from him on all sides. The two old rings that thus date back at least some three hundred years, and probably very much longer, appear to be concentric with the planet and, with the exception of some partial divisions in them, they present apparently a continuous surface, like that of the planet itself. It was supposed at first, that they were solid bodies like the planet, in the shape of thin flat circular discs of nearly uniform dimensions on all sides, and suspended concentrically over the planet's equator. They were also soon found to revolve about the planet as his satellites do, and in the same direction.

Before the application of Newton's law of gravitation to the motions of the heavenly bodies, no one knew any reason why these rings might, or might not, thus continue to revolve about the planet for all time, past or future. But La Place first in applying this law to the rings and the planet, found that if the rings were uniform, homogeneous solids, they could not forever revolve concentrically around the planet, except in the absence of all disturbing forces, which might tend in the slightest degree to separate their centers from that of the planet. He showed that they posessed an *unstable equilibrium of revolution* around the center of the planet, which might be maintained for any length of time, if there was nothing to disturb it; but that the attraction of one of the satellites, or of the sun, or of one of the other planets of the solar system, and especially of Jupiter at his nearest approach, would necessarily tend to separate the centers of the rings from that of Saturn. In this case, if the rings were homogeneous solids, of uniform dimensions in all their parts, La Place showed that the mutual attraction of Saturn and of the several parts of the ring would cause their centers to separate more and more, until the nearest portion of the ring should be precipitated violently upon the

body of the planet and the ring would necessarily be shattered by the collision. Moreover this catastrophe would take place in a very short time. As the separation of the centers would proceed with continually accelerated velocity, perhaps a month would be more than enough to ensure the destruction of the rings. But they have endured now for hundreds and very probably for thousands of years. Hence there must be some means of counteracting this destructive effect of all the attractions to which the rings are subject, and of maintaining their *stable equilibrium of motion* about the planet. LaPlace was still disposed to admit the solidity of the rings. But then, notwithstanding appearances and measurements to the contrary, he denied that they could be uniform in dimensions, or in the density of their several parts. He thought that, if the constitution in different parts were such that their centers of gravity were not at the center of Saturn, then these centers of gravity might continue to revolve around that of the planet, in the same manner as a satellite, and that the *stability of equilibrium* of their motion might thus be preserved. This was, however, merely an opinion of LaPlace, but without any demonstration on this particular point.

The subject remained in this condition until between forty and fifty years ago, when Mr. George P. Bond was examining Saturn's rings with the then new and great telescope of the Harvard Observatory. He saw new divisions and openings in the rings which led him to examine the records of older astronomers, to see if they had made similar observations. He found in the observations of European and other astronomers of the preceding half century and later, many records of the markings and changes which he had himself observed, such as temporary divisions in certain parts of the rings, and of their subsequently closing up. From all these observations, including his own, Mr. Bond came to the conclusion that the rings must be of a liquid nature. Moreover, when he compared the mass of the rings as determined by Bessel, with their volume, as obtained from observation, he found that their density was nearly that of water.

This confirmed him in the view that they were probably of a *li*quid nature. LaPlace also had previously determined

the mean density of the rings to be 1.3 times the density of Saturn. Professor Newcomb gives the mean density of Saturn, compared with water, as 0.75; hence LaPlace's determination will make the density of the rings, compared with water, 0.97½, or only 2½ per cent less than the density of water. In this way Mr. George Bond could account for the temporary openings and closings of certain parts of the rings by the freedom of motion of its several parts under the stress of the varying attractions to which it was subjected by the planet Saturn, and the satellites, and other bodies of the solar system. But this was not all that Mr. Bond did. LaPlace had left his opinion, without demonstration, that the stability of equilibrium of the rings might be maintained if they were *irregular solids*. But Mr. George Bond showed that *no solid ring* could be maintained in stable equilibrium.

On this part of the subject, Professor Newcomb in his Popular Astronomy, has the following statement:

"The question was next taken up in this country by Professors Pierce and Bond. The latter started from the supposed result of observations—that new divisions show themselves from time to time in the ring, and then close up again. He thence inferred that the rings must be fluid, and to confirm this view, he showed the impossibility of even an *irregular solid pair of rings* fulfilling all the necessary conditions of stability and freedom of motion. Professor Pierce taking up the same subject from a mathematical point of view, found that no conceivable form of irregular solid ring would be in a state of stable equilibrium; he therefore adopted Bond's view that the rings were fluid. Following up the investigation, he found that even a fluid ring would not be entirely stable without some external support, and he attributed that support to the attractions of the satellites. But as LaPlace did not demonstrate that irregularities would make the ring stable, so Pierce merely fell back upon the attraction of the satellites as a forlorn hope, but did not demonstrate that the fluid ring would really be stable under the influence of their attraction. Indeed now it seems very doubtful," says Professor Newcomb, "whether this attraction would have the effect supposed by Pierce."

Professor Newcomb makes the following additional statement:

"The next, and, we may say, the last important step was taken by Professor J. Clerk Maxwell, of England, in the Adams prize essay for 1856. He brought forward objections, *which seem unanswerable*, against the solid and fluid ring, and revived a theory by Cassini about the beginning of last century. This astronomer considered the ring to be formed by a cloud of satellites too small to be separately seen in the telescope, and too close together to admit of the intervals between them being visible. This," says Professor Newcomb, "is the view of the constitution of the rings of Saturn now generally adopted."

It is to re-examine this question, and to see if there is not still good reason to maintain the view of Mr. George P. Bond, in preference to that of Cassini and of Professor Maxwell, that this paper has been prepared.

In the first place, it will be readily granted that, if the fluid ring be obliged to preserve an *invariable form*, it will be in no better condition, as to a stable equilibrium, than the *solid* ring, which it is agreed that Mr. Bond, Professor Pierce and Professor Maxwell have all proved to be in *unstable equilibrium*

If Professors Pierce and Maxwell have tacitly assumed an *invariable form* for the fluid ring; or if, with Mr. Bond also, they have failed to find an *adequate cause* for any *required change in its form*, so that its equilibrium may be restored to stability, this fact will account for their want of success in establishing the condition of equilibrium of the rings.

In the present paper it is hoped that this important link in the chain of evidence for the ring's stability will be supplied.

Sir John Herschel, in his Outlines of Astronomy, gives the micrometric measures of the several parts of Saturn's two older rings, which were made by Struve, and reduced to the planet's mean distance from the sun. They are as follows:

Exterior diameter of exterior ring	40″.095
Interior diameter of exterior ring	35 .289
Exterior diameter of interior ring	34″.475
Interior diameter of interior ring	26 .668
Interval between planet and interior ring	4″.339
Interval between the rings	0 .408

Thickness of the rings, not exceeding 250 miles, which is equivalent to 0.00666 of Saturn's equatorial radius.

The thickness of the rings Herschell concluded from their total disappearance in his telescope in 1833, while that telescope would distinctly show a line one-twentieth of a second of arc in breadth. Hence the thickness was less than 0″.05.

Professor Newcomb, in his Table of Elements, on page 528 of his Popular Astronomy, gives the angular diameter of Saturn at distance unity, or at the earth's mean distance from the sun, equal to 162″.8. Hence, at Saturn's mean distance from the sun, his equatorial diameter would be $\frac{162''.8}{9.5388} = 17''.067136$. Therefore Saturn's *equatorial radius*, at his mean distance, is 8″.5336.

If, therefore, we take Saturn's equatorial radius as the unit to measure the distances from his center to the several parts of the two older rings, as well as the distances of his satellites, we shall find the following results:

Distance to outside of exterior ring	2.355
Distance to inside of exterior ring	2.068
Breadth of exterior ring	0.287
Distance to outside of interior ring	2.020
Distance to inside of interior ring	1.563
Breadth of interior ring	0.457
Interval between planet and interior ring	0.059
Interval between the rings	0.048
Thickness of the rings not greater than	0.00666

Professor Newcomb gives a table of distances of Saturn's Satellites in terms of his equatorial radius as a unit, which agrees to the nearest tenth of a unit, in all but two of the satellites, with a similar table given by Sir John Herschel. Professor Newcomb's table, which will here be adopted, is as follows:

1st	Satellite,	Mimas,	distance	from	Saturn	3.3
2d	"	Enceladus,	"	"	"	4.3
3d	"	Tethys,	"	"	"	5.3
4th	"	Dione,	"	"	"	6.8
5th	"	Rhea,	"	"	"	9.5
6th	"	Titan,	"	"	"	20.7
7th	"	Hyperion,	"	"	"	26.8
8th	"	Japetus,	"	"	"	64.4

The effect of Saturn's sixth and largest satellite, Titan, in removing the center of the exterior ring from the center of the planet, may be determined by the same kind of computation as that by which the tides in the waters of the earth are produced. This is done by taking account of the *difference of attraction* which the moon exerts on the earth as a

whole, at its center, and on the waters nearest the moon and farthest from her. For it is evident that the satellite, Titan, will attract the part of the ring nearest to him more strongly than he will attract Saturn's mass condensed at the planet's center; and that he will attract the more distant part of the ring less strongly than he will attract Saturn's mass. Hence the *difference of attraction* by the satellite, with regard to Saturn and the two opposite parts of the ring, will be entirely similar to the action of the moon in producing our tides.

In order to determine the *nature* of these effects, it will not be necessary, perhaps, to develop, in an entirely rigorous form, the whole mathematical theory of the tides; but only to recall a few points in the simplest form of this theory.

Taking the earth's equatorial radius as the unit of distance, the moon's distance from the earth's center may be stated as 60. Its distance from the waters nearest the moon will then be 59; and from the waters farthest from the moon, in the opposite hemisphere, the distance will be 61.

If m = the moon's mass, her accelerative attraction on the earth as a whole is the same as though the earth's mass was all condensed into its center, and will be expressed by $\frac{m}{60^2}$. The moon's attraction on the waters nearest to her will be expressed by $\frac{m}{(59)^2}$; and her attraction on the waters furthest from her by $\frac{m}{(61)^2}$. Hence the waters directly under the moon, and nearest to her are drawn away from the earth's center by the moon's disturbing force f_1,

$$f_1 = \frac{m}{(59)^2} - \frac{m}{(60)^2} = \frac{119m}{(59)^2 \times (60)^2} \text{ or, } f_1 = \frac{m}{105308}.$$

This force tends to diminish the gravitation of the water nearest the moon towards the earth's center, and to cause it to swell upwards towards the moon, and away from the earth's center, or to raise a high tide directly under the moon.

But the *difference* of the *moon's* attraction on the earth, and on the waters farthest from her, in the opposite hemisphere, is

$$f_2 = \frac{m}{(60)^2} - \frac{m}{(61)^2} = \frac{121m}{(60)^2 \times (61)^2} = \frac{m}{110707}.$$

This force, f_2, tends to draw the earth's center away from the most distant waters, or it is equivalent to driving these waters away from the earth's center, and so tends to raise another high tide in this opposite hemisphere.

To obtain the entire momentary effect of the moon in tending to produce these high tides in the opposite hemispheres of the earth, it would be necessary to take account of the sum total of such disturbing forces in each hemisphere caused by the action of the moon on the earth and on each particle of water wherever situated in the two hemispheres. But, as many of these forces in each hemisphere are nearly equal to those we have expressed by f_1 and f_2, and as they gradually approach to equality in proportion as we recede from the points nearest the moon, and most distant from her, it is natural to suppose that at least a certain rough approximation to the ratio of all those forces in the one hemisphere to those in the other, will be obtained by taking the ratio of the single forces, f_1 and f_2. This ratio is

$$\frac{f_1}{f_2} = \frac{110707}{105308} = 1.0512.$$

It follows from this *entirely arithmetical computation* that the *moon's disturbing force* on the waters nearest to her, is about five per cent greater than that which she exerts on the waters most distant from her in the opposite hemisphere. By taking the actual summation of the greater portion of these forces in the two hemispheres, in a manner that will be subsequently given in this paper, I find that the excess of the moon's action on the waters supposed to cover the whole of the nearer hemisphere, over her action on the waters supposed to cover in like manner the more remote hemisphere, is about two per cent of the whole action on the nearest hemisphere. The consequence is that the moon ought to raise a tide in the hemisphere immediately under her about two per cent higher than in the opposite hemisphere at the same time. The average lunar tide in the open ocean, at great distances from the continents, near any small island, has been ascertained not to exceed about two feet. Two per cent of this height is only forty-eight hundredths of an inch, less than half an inch. It is not surprising then that this small difference is not generally recognized. Yet it exists, and a further consequence of it is that the earth's center of gravity is

thereby slightly removed towards the moon, because of the greater bulk of water drawn towards our satellite.

The sun also affects the tides in our oceans, in the same manner as the moon does; but, owing to his much greater distance, or to his much smaller *difference of distance* from the earth's center and from its two opposite hemispheres, compared with his whole distance, his greatest tidal disturbance of the oceans is not more than half that produced by the moon; and the difference of his action in the two hemispheres is very much smaller than that before noticed, though it is of the same kind. Let us next examine the disturbing action of the sun on the exterior ring of Saturn, arising from the *difference of his attraction* on the planet, and on the two halves of the ring situated on opposite sides of Saturn with reference to the sun.

Saturn's mean distance from the sun, in terms of the greatest radius of the exterior ring, taken as the unit, is about 10285. Hence the sun's distances from the nearest and most remote points of that ring are 10284, and 10286.

If therefore f_1 and f_2 are now the disturbing actions of the sun, in reference to these parts of the ring and Saturn's center, and if M= the sun's mass, it follows that

$$f_1 = \frac{M}{(10284)^2} - \frac{M}{(10285)^2} = \frac{20569\,M}{(10284)^2 \times (10285)^2}$$

$$f_2 = \frac{M}{(10285)^2} - \frac{M}{(10286)^2} = \frac{20571\,M}{(10285)^2 \times (10286)}$$

$$\text{Hence } \frac{f_1}{f_2} = \frac{20569}{20571} \times \left[\frac{10286}{10284}\right]^2 = 1.00029$$

Hence the sun tends to draw the nearest point of the exterior ring away from Saturn's center by a certain small disturbing force, and to draw away Saturn's center from the farthest point of the ring, or apparently to drive this part of the ring away from Saturn's center, by another small disturbing force. But the former force exceeds the latter by less than three hundredths of one per cent of the former. By taking the sum total of all the principal disturbing forces of the sun on the two opposite parts of the ring, I find the ratio to be $\frac{f_1}{f_2} = 1.000115$, or about one hundredth of one per cent more on the nearer, than on the more remote half of the

ring. The tendency of the sun's disturbing forces is therefore to draw the center of this ring very slightly away from Saturn's center toward the sun. At the same time, *if the ring be liquid*, this action of the sun will tend to produce high tides in the portions of the ring nearest to, and farthest from the sun. But the former tide will exceed the latter by about *the same one hundredth part of one per cent of the nearest tide.*

The action of the sun on the inner ring will be of the same kind but smaller in amount, because the *difference* of *distance*, upon which it depends, is smaller.

Let us next examine the disturbance of the exterior ring by the planet Jupiter when nearest Saturn. The nearest mean distance of Jupiter from Saturn, in terms of the greatest radius of the exterior ring, is about 4675. Hence, if m = Jupiter's mass, and f_1 and f_2 are respectively the disturbing actions of Jupiter on the nearest and farthest points of the ring, it will follow that,

$$f_1 = \frac{m}{(4674)^2} - \frac{m}{(4675)^2} = \frac{9349m}{(4674)^2 \times (4675)^2}$$

$$\text{and } f_2 = \frac{m}{(4675)^2} - \frac{m}{(4676)^2} = \frac{9351m}{(4675)^2 \times (4676)^2}$$

$$\therefore \frac{f_1}{f_2} = \frac{9349}{9351} \times \left(\frac{4676}{4674}\right)^2 = 1.0034.$$

But if we take the sum of all the principal disturbing forces of Jupiter on the two opposite halves of the ring, nearest to him, and farthest from him, then the above ratio will become 1.00025. Hence Jupiter's action on the nearest half of the ring exceeds that on the opposite half, by about one fortieth part of one per cent of the former, when nearest to Saturn. By this amount Jupiter tends to draw away the center of the ring from that of Saturn; and *by the same amount* he tends to produce a greater tide in the nearest half of the ring then in its opposite, *provided the ring be fluid.*

It is unnecessary to take account of the actions of the other planets on the rings, since their action is very much less than that of Jupiter, but necessarily of a similar nature.

TO BE CONTINUED.

ON THE DETERMINATION OF THE BRIGHTNESS OF STARS BY MEANS OF PHOTOGRAPHY.*

EDWARD S. HOLDEN.

Dr. Charlier, assistant in the Observatory at Stockholm, has prepared a memoir† on the use of photography in determinations of the brightness of stars, which has been published by the Astronomical Society of Germany, and dedicated to the Pulkowa Observatory, on the occasion of the fiftieth anniversary of its foundation, August 19, 1889.

The subject treated is so new and so important that it will not be out of place to give a brief review of Dr. Charlier's excellent treatise here and to add some general considerations on the same question. The importance of this subject will be obvious, when we consider that within the next decade we may expect to have at least two sets of photographic maps, covering the whole sky from pole to pole, and including millions of stars down to the fourteenth magnitude.

Besides these systematic maps, hundreds of charts of special regions will be made. Each star on each of these maps will have impressed its image on a negative plate as a disc of measurable size. Hence the magnitude of each and every star *can* be determined, if necessary, and when the catalogue of the stars to the eleventh magnitude, also proposed by the Congress, is constructed, the magnitude of each one of these two million stars *must* be given.

There are two imperative questions to be settled before the principles on which this great work is to be done can be considered to be established. The first and more special question is, What is the relation between the diameter of the photographic image of a star (d) the aperture and focus of the telescope employed (a, f) and the exposure time (t), and what is the relation between the (photographic) brightness of a star and the diameter of its image? Having satisfactorily determined the relations just named, the second and more general question presents itself, namely: On what fundamental principles ought the photographic magnitudes of the stars to be assigned?

* Notes prepared by the staff of Lick Observatory.

† *Ueber die Anwendung der Sternphotographie zu Helligkeitsmessungen der Sterne, von C. V. L. Charlier. Publication der Astronomischen Gesellschaft, XIX. Leipzig, 1889, 4to, (pp. viii, 31).*

These two questions are not treated separately in the work before us. Its second paragraph states the problem of the photographic photometry of stars as follows: It is "to determine the function which gives the relation between the size of the photographic image and the photographic brightness of the star, and to determine the constant quantities in this function *in such a manner that the resulting photographic brilliancies shall correspond accurately throughout with the brilliancies determined visually.*"

In my judgment, this is by no means the problem of stellar photographic photometry. It is impossible, in general, to fulfil that portion of the above statement which I have printed in italics. The difference between the photographic and the photometric magnitudes of *Aldebaran*, for example, is more than one and one-half magnitudes, and so with other stars. We may leave this part of the question for the moment, and proceed to give a brief analysis of Dr. Charlier's memoir, laying stress principally on the novel portions of his work.

The observations which he discusses were made with a photographic lens by Steinheil of 3.19 inches aperture and 39.37 inches focus ($\frac{f}{a} = 13$). The plates took in an area of twenty square degrees. The images were satisfactory over a field of about three degrees in diameter. Stars to eighth magnitude, inclusive, left trails. The plates employed were made in Lyons, by Lumière. Four plates are discussed. All were exposed on the *Pleiades*, as follows: No. 2, $t = 13m$; No. 4, $t = 2h$; No. 24, $t = 1h\ 30m$; No. 26, $t = 3h$. The plates were exposed at very different altitudes, and no account is taken of absorption of light by the atmosphere.

Dr. Charlier finds two defects in the plates; first, bright rings round the larger stars, which he proves to be due to reflections from the back of the plate (the well-known halation images); and, again, false stars. He finds no less than fifty-six such false stars on his plate No. 26. They were probably due to defects in the manufacture of the plate itself.

As subjects for experiments he chose the *Pleiades*, because their photometric magnitudes are accurately determined and also because they afford a variety of magnitude within a comparatively small area.

Although he does not expressly mention the fact, the

Pleiades have the special advantage for his purpose of being all of the same spectral type. A region containing many very red or many very blue stars would have given a corresponding number of anomalous results, which are avoided by choosing a group of stars of one type. The diameter of each star on each of the four plates was measured. Calling H the brightness of a star, and m its magnitude and 0.4 the light ratio, Dr. Charlier starts with the formula

(1) . . . . . $H = (0.4)m$

That is, he assumes that the brightness of a first magnitude star ($m = 1$) is 0.4. It is better to write this formula, I think,

(2) . . . . . $H_m = (0.4)m - 1$

which for $m = 1$ gives $H_1 = 1$. Assuming the equation (1), however, and further assuming that when d is zero, H must be zero, he finds

(3) . . . . . $m = a - b \log d$

Here it may be remarked that, in fact, H is not necesarily zero for $d = 0$, because all stars below a certain brightness will fail to produce an image on the plate, no matter how long the exposure may be—for any practical exposure time. The brightness of a star must be above a certain finite limit in order to produce any impression at all. The assumption is sufficiently accurate, however, for the purpose in hand. The relation between exposure-time and diameter of star image is next determined from a series of exposures on *Polaris*, assuming the form

(4) . . . . . $d = d_0 \cdot t^k$

That is, that the diameter of the star-image varies as the k^{th} power of the time. From *Polaris* (two plates) the values of k are 0.243 and 0.249; from a star 5th mag. k results 0.243,—hence, the numerical value of the diameter of the star-image varies as the fourth root of the time or

(5) . . . . . $d = d_0 \sqrt[4]{t}$.

This formula shows that the diameter d will be doubled when the exposure t is increased sixteen-fold.

If there are no limits to the formula it also shows that, for the telescope and plates employed, an exposure of one-sixteenth second would give a preceptible image. Without

considering the question of the *range of sensitiveness* of plates I may state it as my opinion that the formulæ of Dr. Charlier and those of Professor Schaeberle (*Publ. Ast. Soc. Pacific, No. 4*) can (at present) be applied safely only to *over-exposed* stars, and that there is a superior limit also beyond which they are no longer applicable. Both Dr. Charlier and Professor Schaeberle have found that the stars with the longest exposure are best fitted for the determination of magnitude.

We may now quote, without further remark, the final formula to which Dr. Charlier is led, which gives the relation between m (star's magnitude), d (diameter of star-image on plate), and t (exposure time). It is

(6) . . . . $m = A + B \log d + C \log t$

In the particular plates in question the constants A, B and C are

$$A = +17.2 \qquad B = -6.75 \qquad C = +1.69$$

A, B and C are proved to be constant on the four plates in question; t is expressed in minutes.

From the formula (6) the photographic magnitudes of 52 of the brighter stars in the Pleiades were computed and compared with the photometric magnitudes of the same stars as determined by Dr. Lindemann, at Pulkowa. (Table III.)

The mean difference between the photographic and photometric magnitudes is $\pm$ 0.22 mag. The differences occur 0.6 mag. (twice), 0.5 (twice), 0.4 (four times), 0.3 (twelve times), 0.2 (twelve times), 0.1 (ten times), 0.0 (seven times). Two stars are either variable or red. The individual results for the photographic magnitudes from the four plates agree well. The mean difference is 0.10 mag. The largest difference is 0.4 (occurring twice).

Dr. Charlier makes the important remark that the red stars, etc., which are thus discovered in the group of the Pleiades, are very suitable for a determination of its parallax, since they differ in spectral type, and are therefore *presumably* not members of the group. A few moments' examination with a small spectroscope will, however, be a surer indication in similar cases.

Section III of the memoir is devoted to a comparison of the results of the Stockholm photographs with those ob-

tained by Professor Pickering, at Harvard College, and by Dr. Scheiner, at Potsdam. The linear formula deduced by the latter is shown to be inferior to the logarithmic form adopted by Dr. Charlier; and in Table IX it is shown that the systematic differences between the results at Harvard College and at Stockholm are likely to be due to constant errors in the H. C. O. results. In all this discussion, as has been said, the effect of atmospheric absorption is omitted, as it has been in all previous publications of the kind. It is of considerable amount, however.

Section IV of the memoir is chiefly concerned with a comparison between the photometric magnitudes given by Wolf, of Paris, for 571 of the *Pleiades* stars and the photographic magnitudes of the same stars derived from one plate (only) taken at Stockholm. Twenty-eight of Wolf's stars do not appear on this plate; *en revanche*, it contains more than 100 stars not in Wolf's catalogue. In passing, we may remark that the single Stockholm plate made in three hours has a value at least comparable with the chart of M. Wolf, which was the result of many months of labor. It is worth while to remark here that it is highly desirable for the present to make every result derived by photography depend on two negatives at the very least. A comparison of the scales of Wolf and Charlier closes this section and concludes the important work.

We may now say that the present memoir and that of Professor Schaeberle, previously cited, have fixed the form under which discussions of this character must be made in future. For every telescope a relation between the diameter of a star image and the corresponding exposure must be deduced in the form $d = \varphi(\log t)$.

The constants of this formula will vary with the aperture, focus, plate, site, and with the spectral type of the star, and will probably be applicable only within certain limits of absolute brightness and within certain limits of exposure time.

The memoir of M. Charlier is an excellent example of the method of discussion which must be adopted to determine this function for all cases where the prime object is to make the photographic magnitudes harmonize as nearly as possible with the photometric. The real fundamental question is, however, Should any endeavor be made to harmonize them?

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I proceed to discuss this point as briefly as possible, in the light of our present knowledge, since it is the most important question remaining open for settlement.

Establishment of the Present System of Visual Magnitudes.

Let us consider, very briefly, the history of the introduction of the present system of visual magnitudes. The main epochs in this history are very few. The first is that of Ptolemy (A. D. 150,) who arbitrarily assumed the brightest stars to be of the *first*, the faintest which he could see, to be of the *sixth* magnitude. The other stars were divided into classes of 2d, 3d, 4th, 5th, etc., magnitudes. The second great event in this history is the publication of the *Uranometria Nova* by Argelander, in 1843. He adopted the general rules laid down by Ptolemy, and followed by Sufi, Tycho and Bayer. The brightest stars were called first magnitude, the faintest visible to the naked eye were called sixth magnitude. Stars of the classes 2, 3, 4, 5, etc., were intermediate. By Fechner's law, it necessarily followed that equal differences of sensation corresponded to equal *ratios* of light; or that the light of a star of m^{th} magnitude must be $\frac{1}{\delta}$-th part of the light of a star one magnitude brighter $(m-1)$. Measures of this *light ratio* δ show its numerical value to be 0.4 very nearly, omitting all questions of small variations, etc.

The *Durchmusterungen* of Argelander, Krueger, Schoenfeld and Thome will determine the visual magnitude of every star in both hemispheres as bright as the tenth magnitude by this same scale. That is, if the brightness of a star of the first magnitude is unity, the brightness of a star of the m^{th} magnitude is

(7) . . . . . $H_m = (\delta)^{m-1}$ where $\delta = 0.4$

The universal practice of modern observers has extended this scale from the tenth down to the sixteenth or seventeenth magnitude (the faintest stars now visible in the largest telescopes). Thus the *accidental* choice of the sixth magnitude as the limit of the naked-eye stars by Ptolemy has fixed the light-ratio and the practice of all astronomers with regard to visual magnitudes for all time to come. It is to be noted that if Ptolemy's work on visual magnitudes were to be done again *de novo*, and absolutely independently, the

method chosen would be essentially the following: *One* standard star would be chosen (Polaris in our hemisphere). This star would be compared with a selected group of stars, and the fact of the constancy (or the law of the variation) of its light during the course of the observations would be established. Every other star would be compared with Polaris, either directly or indirectly, and its relative light determined. Some convenient magnitude would be arbitrarily assigned to Polaris, and some convenient, light-ratio would be *arbitrarily* assumed. The magnitude of any and every star would then be deduced from the measured ratio of its brightness to that of *Polaris* by a formula like our (7) in which the numerical value of δ would be assigned on grounds of convenience alone. It is very likely that the value $\delta = 0.4$ would be again chosen because the tenth part of a magnitude (easily written with one place of decimals), thus defined, is about the limit of the reception of the most highly trained human eye.

Such, I conceive, would be the process adopted if the whole question of visual magnitudes was entirely open, and if a Congress of Astronomers were called in 1890 to decide on the proper methods to be followed in fixing the visual magnitudes of the stars anew or for the first time. The process is simple; it is complete: it is logical; it is sufficiently accurate for all conceivable uses to which visual magnitudes are to be put. The use of a visual magnitude assigned to a star is chiefly to determine its brightness at one epoch, so that observations at other epochs will determine whether there have or have not been changes in its light. It is from celestial bodies which are subject to change, and chiefly from these, that we can hope to learn anything of the nature of celestial bodies in general. A secondary convenience in having a magnitude assigned to a star is to aid in identifying, classifying and describing it.

Establishment of a System for Determining Photographic Magnitudes.

The International Congress of Astronomers will have to decide the question as to how to define the photographic magnitude of a star. They will soon be in possession of plates on which millions and millions of stars have im-

pressed themselves. The diameter of each one of these stars can be measured. The photographic brightness of each one of these stars relative to the photographic brightness of Polaris (for example) can be readily determined. What *magnitude* shall be assigned to one of these stars? Dr. Charlier's answer to this question has already been given. He would assign to each one of a group of stars a photographic magnitude, deduced on the principle that the mean deviation of their photographic magnitudes from their visual magnitudes should be as small as possible. If the same star occurs in two or more different groups, it will certainly have different magnitudes assigned to it, according as one or the other set of standards is employed. The same method has been followed by Mr. Espin and by the Harvard College Observatory in all of its many important publications on this question, notwithstanding the fact that (owing to the color of a star) the photographic and visual magnitudes not infrequently differ by at least *two whole magnitudes*. That is, if the visual brightness be expressed by 1.00, the photographic brightness of the same star may be no more than 0.16, or only one-sixth part. Such anomalies must, in the nature of things, constantly appear for a considerable percentage of the stars. A tolerable agreement is possible for perhaps eighty per cent of the larger stars, and even here there will be small persistent differences. For those remaining, the disagreement will be more or less marked, according as the spectral type of the star in question varies more or less from the average type. The reason of this is well known. The eye is sensitive to rays which fall between the Fraunhofer lines B and G (approximately) of the solar spectrum. The maximum brilliancy to the eye is somewhere near the line *b*. The photographic plate is sensitive to rays falling between F and N of the solar spectrum (approximately). The plates now in use are sensitive in the highest degree to rays of about the wave length of the line G.

Whenever we have a group of say 500 stars, whose spectra are nearly all of the same type (as the Pleiades, for example,) we can measure for each star the relative energy of the light in the portion of its spectrum between B and G (by the eye), in that between F and N (by the photographic plate), and, *as the energy is distributed according to*

the same law in the spectrum of each star of the group, we can determine constants of reduction which *will* make the photographic magnitudes of the various stars agree well with their visual magnitudes. If, however, two hundred of the stars are very red, one hundred very blue, and two hundred of the ordinary type, it is, in the nature of things, impossible to bring the photographic and the visual magnitudes to a good agreement. The very red stars will always appear brighter to the eye than they do on the plate, and the very blue stars will always appear fainter to the eye than on the plate, and there is no process of reduction which will smooth away a difference in their magnitudes which is inherent in their nature. If there were such a process, it would be most unwise to employ it. When I see that a star is of the visual magnitude 1 and the photographic magnitude 2.5, I at once learn something of the nature of this star's spectrum, and so in like cases.

It therefore seems to be a rational and useful plan to leave out all consideration of the visual magnitude of stars in determining their photographic magnitudes. A simple and most satisfactory method of procedure would be to assume Polaris as the standard star of the whole sky, and to fix its magnitude (when in the zenith of a station at sea level) at 2.00, once for all; to select a set of secondary standards, distributed around the equator, and to determine the brightness of each one of these stars in terms of that of Polaris (a proof of the constancy of the light of Polaris being thus attained). Important groups like the Pleiades, etc., would also have their brightness determined in terms of that of the standard. The brightness of the principal Southern stars should also be fixed in terms of Polaris indirectly through the Pleiades, etc. A light ratio should be selected on grounds of convenience alone and the photographic magnitude of every star should be determined by an equation like our equation (7) in terms of a single standard star with a definite light ratio.

If this program were to be followed, we should simply have to add to our star catalogues another column headed "Photographic Magnitude," which would immediately follow the column "Visual Magnitude." The agreement or disagreement of the two numbers would tell us something of

the nature of the spectrum of each star. In order to have the work exact, it would be necessary that all the stars should be photographed on one kind of plates, as is now done by the Harvard College Observatory, and as will be done by the International Photographic Congress. The photographic Southern Durchmusterung might for convenience have its magnitudes expressed in visual units, though the DM of the Cordoba Observatory will make this unnecessary, and will, in fact, make it distinctly to the advantage of science if the photographic DM is made entirely photographic. The International map of two million stars to the eleventh magnitude should, in my judgment, give photographic magnitudes alone. I can conceive of no advantage to be gained by determining the approximate visual magnitudes of these millions of stars at all comparable with the labor involved. In any event, it would seem that the photographic magnitudes should be given whether the visual magnitudes are or are not.

Such, it appears to me, are the general principles which should govern in the determination of star magnitudes by photography. I have set them forth because no amount of discussion at this stage can be called superfluous. After the International Congress has once settled its methods of procedure, it will be the duty of all coöperating Observatories to conform to the spirit and to the letter of the methods finally adopted. As long as they are not yet adopted any suggestions, however simple, cannot fail to be of use.

The Lick Observatory is endeavoring to make a modest contribution to the general subject of which we have spoken. Professor Schaeberle has made observations at Mount Hamilton (4209 feet above sea), and will make observations at Cayenne, South America, (nearly at sea level), to determine the photographic atmospheric absorption at zenith distances between 0° and 70° or 75°. He has already compared the Pleiades and other stars with Polaris, and will compare the principal southern stars with the Pleiades, etc. In this way, his observations, if successful, will enable us to transfer the standards of the Northern Hemisphere into the Southern.

The immense work now in progress in both hemispheres under the auspices of the Harvard College Observatory will

afford material for a thorough discussion of the whole subject. The contributions of Dr. Charlier and Professor Schaeberle have established the final form under which special discussions of this kind must be made. The only part of the subject remaining for settlement is that which relates to the establishment of the fundamental principles on which the final methods of reductions are to be based. I have endeavored, in what precedes, to set forth what seems to me to be a satisfactory system, at once simple and comprehensive.—*From advance sheets of Publications of the Astronomical Society of the Pacific.*

ON THE LIMIT OF SOLAR AND STELLAR LIGHT IN THE ULTRA-VIOLET PART OF THE SPECTRUM.*

WILLIAM HUGGINS, D. C. L., LL. D., F. R. S.

It has been long known that the solar spectrum stops abruptly, but not quite suddenly, at the ultra-violet end, and much sooner than the spectra of many terrestrial sources of light. The observations of Cornu, of Hartley, and, quite recently, of Liveing and Dewar, appear to show that the definite absorption to which the very rapid extinction of the solar spectrum is due, has its seat in the earth's atmosphere, and not in that of the sun; and that, consequently, all ex-terrestrial light should be cut off at the same place in the spectrum.

During several years I have attempted to obtain the limit in the ultra-violet for stellar light here, but as it was necessary to make use of a bright star at a high altitude, and at a time when the atmosphere was very clear, it was not until September 20th, 1888, that I was able to obtain a result which seemed to me to be satisfactory.

On that night three successive photographs of Vega,† with increasing exposures, were taken on the same plate. The first spectrum was exposed for 10 minutes, the second for 20 minutes, and the third spectrum nearly four times as long, namely, for 70 minutes.

A comparison of the extent of the second spectrum due to an exposure of 20 minutes with that of the third spec-

* Read before the Royal Astronomical Society.

† We are sorry that our engraver is unable to reproduce the fine engraving that should acompany this article.—Ed.

trum, to which an exposure of 70 minutes was given, leaves no doubt that the latter spectrum has reached the limit imposed by atmospheric absorption, and has not stopped in consequence of an insufficient exposure of the plate.

The original plate has been enlarged about four times; and a spectrum of magnesium and calcium, taken with the same apparatus, and enlarged simultaneously with the plate of stellar spectra, has been placed above to serve as a scale.

As the spectra are prismatic it is not possible to indicate the wave-lengths in a scale of equal parts. A short scale only is placed over the spectrum where the light of Vega ends.

The spectroscope with which the spectra were taken is furnished with a prism of Iceland spar and lenses of rock crystal, and a mirror of speculum metal was used to condense the light of Vega upon the slit.

It will be seen that at my Observatory* the light of Vega at about λ 3000 is abruptly weakened, and then continues as a very faint line to the point of apparent extinction at λ 2970.

Numerous solar spectra taken here during the last four years with the same spectroscope show an average abrupt weakening at about λ 3000, and an apparent total extinction at about λ 2985.

On two occasions only the very faint weakened spectrum could be traced as far as λ 2970.

The abrupt narrowing of the spectrum at the end towards the red is produced by the rapid falling off of sensitiveness of the silver bromide for light of increasing wave-length.

The increase of breadth of the spectra with increase of duration of exposure is due to the same causes, optical and photographic, which produce the increase of diameter of stellar disks on the photographic plate with longer exposures, when a reflector is used. At *h* the breadths of the spectra, having 20 minutes and 70 minutes exposure respectively, are 0.06 inch and 0.105 inch.†

* Elevation of the Observatory 177 feet above mean sea level. Barometer about 30.03 inches at the time of observation.

† The law of increase of size of image with exposure is not as yet accurately defined. Bond found that the diameter of star-disks varied nearly as the square root of the time of exposure. Pritchard, using a reflector, found a law near the fourth root; and Mr. H. H. Turner has recently found a law very near the cube root for plates taken with a photoheliograph object glass ('Astron. Soc. Month. Not.,' vol 49, p. 292).

In 1879 Cornu* made experiments on the limit of the spectrum with reference to the altitude of the place of observation. On the Riffelberg, at an elevation of 8414 feet, the spectrum reached to λ 2932, while at the lower elevation of Viège, 2163 feet, the spectrum stopped at 2954. He concludes that the absorption is due to the gaseous constituents, and not to aqueous vapor in the atmosphere.

In 1881† Heartley stated that an amount of ozone proportional to the average quantity in a vertical column of the atmosphere, caused an absorption similar to that observed in the solar spectrum, namely, terminating about λ 2950.

Quite recently Liveing and Dewar have made some important experiments on the absorption-spectrum of large masses of oxygen under pressure.‡ They state that with a tube 165 cm. long and a pressure of 85 atmos., oxygen appeared to be quite transparent for violet and ultra-violet rays up to about λ 2745. From that point the light gradually diminished, and beyond λ 2664 appeared to be wholly absorbed.

In some later experiments with a steel tube 18 metres long and a pressure of 90 atmos., oxygen produced complete absorption above P, *i. e.*, λ 3359.2.

M. Janssen, from his observations on the Alps, concludes that both the bands which follow the law of the square of the density, and the dark lines obeying a different law of information, which are due to oxygen in the solar spectrum, are produced exclusively by the earth's atmosphere—"L'atmosphère solaire n'intervient pas dans le phénomène."§

THE ASTRONOMICAL SOCIETY OF THE PACIFIC—A HANDSOME GIFT.

A meeting of the Astronomical Society of the Pacific was held November 30, at the hall of the Academy of Sciences, corner of Dupont and California streets. In the absence of President E. S. Holden, William M. Pierson occupied the chair. Every seat in the large hall was occupied. The fol-

* "Sur l'Absorption Atmospherique des Radiations Ultra-violettes," 'Journ. de Physique,' vol. 10, 1881.

† "On the Absorption Spectrum of Ozone, and on the absorption of solar rays by Atmospheric Ozone," 'Chem. Soc. Journ.,' vol. 39, 1881, pp. 57, 111–129.

‡ 'Chemical News,' vol 58, p. 163, and 'Phil. Mag.,' September, 1888, pp. 286–290.

§ 'Comptes Rendus,' vol. 107, p. 677.

lowing named candidates were elected members of the society:

O. C. Hastings; Charles M. Bakewell; Warren Olney; Frank James; Dr. J. H. C. Bonte; Hon. Horace Davis; William G. Raymond; James R. Remberton; Augustus F. Knudson; C. D. Perrine; F. B. Rodolf; Miss Agnes Clerke, of London, England; Garrett P. Serviss, Brooklyn, N. Y.; Miss C. W. Bruce, N. Y. City; John Gamble; Charles S. Aikin; Charles F. Hart; J. J. Herr; Whitney Herr; John H. Yoell; N. E. Beckwith; J. E. Richards; Irving M. Scott; Warren B. E. West; Martha McColeman Ewer.

The principal paper of the evening was read by E. E. Barnard, Lick Observatory, on some photographs of the Milky-Way and other celestial objects, that he had made with a large potrait lens of six inches aperture and thirty-one inches focus, strapped to the tube of the six and one-half inch equatorial of the Lick Observatory, the clock-work of the instrument being controlled by hand, with the slow motion rods at the eye-end. A star was kept bisected by the cross wires in a high power eye-piece on the telescope itself. The additional weight of the camera made it necessary constantly to correct the clock throughout the exposures. With this instrument a negative of the Pleiades was made August 23, of this year, with an exposure of $1^h\ 15^m$. This showed the Merope nebula conspicuously, the sharp prong of nebulosity from Electra, and some of the nebulosity about Maia and Alcyone. A negative of the Milky-Way (right ascension, $17^h\ 57^m$, declination south $18^\circ.9$), was made July 28, with an exposure of $2^h\ 35^m$; another in the region about M 11 on August 2, with exposure $2^h\ 45^m$, and another of the Milky-Way (right ascension $17^h\ 56^m$, declination, south 28°), August 1, with an exposure of $3^h\ 7^m$, and a negative of the great nebula of Andromeda, August 26, with $4^h\ 18^m$ exposure. The paper was illustrated by lantern slides from these plates projected on a large screen by the aid of oxy-hydrogen light. The nebulosity of the Pleiades was very conspicuous, and the beautiful cloud forms of the Milky-Way, with the myriads of stars that they were partly resolved into were strikingly fine. The slide of the great nebula of Andromeda, when first projected on the screen, had a mask over it, with a small hole, representing (to scale) the largest field of the great telescope on Mount Hamilton. This was moved about over the slide showing successive fields of view, upon and around the great nebula, indicating

what could be seen at once in the great telescope; the mask was then suddenly removed and the entire nebula, suspended amidst countless stars, flashed into view. The contrast between the limited space representing the field of the great telescope and the sky as shown by the photographic lens was astonishing in the extreme. This slide shows the great rings of nebulosity that were first proved to exist by Mr. Roberts, of England. By carefully counting areas, Mr. Barnard estimated that on the original plate (8×10 inches) there were distinctly visible no less than sixty-four thousand stars. This entire plate had been reduced to a lantern slide which, upon the screen, brought out peculiarities in the arrangements of the stars that were not even suspected in the original plate. On all these plates the star images were perfectly round.

Mr. A. O. Leuschner also read a paper on the orbit of Swift's comet which he had computed from observations by Mr. Barnard at the Lick Observatory Nov. 20, 21, and 22. He called attention to the small inclination, and to the small perihelion distance which were suggestive of periodicity.

Elements of the comet of Swift (Nov. 16, 1889) by A. O. Leuschner:

$$\begin{aligned} T &= 1889 \text{ Dec. } 11.8493 \\ \Omega &= 306^\circ\ 25' \\ \omega &= 116\ \ 24 \\ i &= \ \ 6\ \ 47 \\ \log q &= 0.0633 \\ q &= 1.1568 \end{aligned}$$

$$\begin{aligned} O - C \\ \delta\lambda \cos\beta &= +\ 1'.2 \\ \delta\beta &= \pm\ 0'.0 \end{aligned}$$

A paper was read by Mr. A. O. Leuschner "On the Determination of the Relation between the Exposure Time and the Consequent Blackening of the Photographic Film." This paper dealt with the investigation of a plate upon which a number of standard squares had been impressed with the standard lamp of the Lick Observatory. These squares had been given exposures of 1*s*, 2*s*, 4*s*, 8*s*, . . . 128*s*. These squares were then compared with each other and with a standard square by means of the wheel photometer of the Lick Observatory. Every precaution was taken to eliminate

errors of observation in the comparisons. An extended series of observations showed that the density was proportional to the time only between the exposures from 2*s* to 8*s*, and with an approximation up to 64*s*; beyond this the density was not proportional to the time. Mr. Leuschner also found that squares of the same exposure time on different parts of the same plate were not equal in density, due, doubtless to the unequal distribution of the emulsion on the plate, and therefore the above results were derived from comparisons with the deduced mean of four squares in each case.

After the papers were read, the chairman surprised the society by stating an offer from one of the members, Alexander Montgomery, Esq., of a present of $2,500. The donation may be used by the society for any purpose, and it has been suggested that it be used for establishing a gold medal to be given annually for the best paper on astronomy to be read before the society. Mr. Montgomery was accorded the hearty thanks of the society.

Including the members elected at this meeting, the society has now 171 active and life members. Its financial condition is also very satisfactory.

NEWS FROM THE NEBULÆ.

PROFESSOR C. A. YOUNG.

On looking over an article on the nebulæ which I wrote for the *Boston Journal of Chemistry* in 1873, I find nothing to change, although there is now something to add. In the first place, several hundred new nebulæ have been discovered within the sixteen years, so that the total number known is at present well above 8,000. The new ones are, of course, for the most part, extremely faint, and some of the most interesting of them are quite *invisible*, even in the largest telescopes,—*i. e.*, the *eye* cannot detect them: but they impress themselves upon the photographic plate, and so come to our knowledge. For instance, within a small area of about ten square degrees, in and near the constellation of Orion, Professor Pickering has found upon his star-plates twelve new nebulæ, where eighteen were catalogued before; and in the

cluster of the Pleiades, where a single faint nebula was known in 1873, the photographs show wisps of nebula attached to nearly all the larger stars, with a considerable number of smaller nebulæ that are isolated.

When one *looks* through a telescope, very little is gained by prolonging the gaze, but upon the photographic plate the action of light accumulates with the time. By merely lengthening the exposure (in some cases for several hours) an eight-inch glass can be made to bring out objects which the eye cannot reach even with an eighteen-inch instrument.

The gain from photography is by no means only, or even chiefly, in the number of new objects; still more important are the new features which the photograph reveals in certain familiar objects which, after long study in the old-fashioned way, were supposed to be fairly known.

The first photograph of a nebula was made by Dr. Henry Draper of New York, in September, 1880. His very first picture (of the nebula of Orion) was by no means poor: but he soon surpassed it, and in March, 1882 a few months before his untimely death, he produced one which is not much inferior to the best we now have. Within the last two or three years, Common and Roberts in England, and Von Gothard in Hungary, have specially distinguished themselves in this line of work. The photographs of the nebula of Orion which Mr. Common has already made with his three foot reflector, are simply magnificent, and it is to be expected that with his new five-foot instrument, now just beginning its serious work, still more remarkable results will be attained.

Mr. Roberts' photograph of the nebula of Andromeda* is not only admirable as a picture, but it has brought out certain new and most interesting facts, which throw a flood of light upon the structure and nature of the wonderful object. Years ago, Mr. Bond, of Cambridge, discovered in this nebula two narrow dark streaks, or "lanes," nearly straight and paralled to its length,—for the form of the nebula is that of an elongated and pretty regular oval, much brighter at the centre. In the new photograph, these "lanes" are seen to be merely the more conspicuous portions of two narrow, oval, dark streaks, or channels, which entirely surround the central mass, and irresistibly suggest

* See frontispiece copied from "Himmel und Erde."

structure like that of Saturn encircled by his rings—an almost perfect exemplification of Laplace's nebular hypothesis. One eminent astronomer, indeed, has gone so far as to suggest that the two small companion nebulæ, which lie one on each side of the main nebula and outside the lanes, are really two half-finished planets. It is worth noting that the photograph shows no trace of the "new star" which in 1885 so suddenly appeared in this nebula, near its nucleus, and then slowly faded away. The fact that the star is not visible upon the plate does not, however, demonstrate its absence; its invisibility may be due simply to the fact that the region of the nebula in which the star is situated is so bright, that the long exposure necessary to bring out the fainter details has greatly overdone this portion of the plate.

One of the most serious objections to astronomical photography lies precisely here: the retina possesses a far greater range of sensibility than the plate. When we look at a nebula, we can see at the same time its brighter and its fainter regions, as well as the glittering stars which here and there are scattered through it. The highest beauty of the nebula of Orion, for instance, lies in the contrasts and gradations between the splendid stars of the "trapezium," the curdled clouds of greenish light which envelope them, and the impressive blackness of the "fish mouth." In the best photographs of this nebula, the stars are mere blotches, and the contrasted darkness of the telescopic background is replaced by a tangle of faint nebulous wisps, which, though in themselves curious and interesting, none the less obscure and befog the familiar outlines.

The photographs produced by Von Gothard (of Hereny in Hungary), about two years ago, have excited much interest, because they were made with a silver-on-glass reflector of only ten inches aperture. Though the pictures are almost microscopic in size,—only a few millimetres in diameter,—they show many things that before had been clearly seen only with the largest telescopes; and they not only show them, but bring them out with increased emphasis and clearness. The so-called "whirlpool nebula" is an example. The photograph does not exhibit, to be sure, such a series of regular spirals, nor such a blaze of light, as the well-

known drawing of Lord Rosse (which, however, lays no claim to minute accuracy); but on the little photograph every real wisp of nebula appears in its true place, and there are numerous "knots," not shown in the old drawing, where the nebulous matter seems to be gathering into globes, besides many streams of minute stars which stand in evident relation to neighboring filaments of the nebula. These stellar streams form a marked feature in nearly all the photographs of the more brilliant nebulæ. They appear to be formed by the collection of the nebulous matter into isolated masses that are denser and more luminous than the rest. Whether this view is correct or not, the star streams are certainly significant.

Mr. Huggins' recent photographs of the spectrum of the nebula of Orion indicate the same thing in a different way. In one of his observations the slit of the instrument was so adjusted that the image of one of the stars of the "trapezium" fell upon it. As a result, the spectrum, as photographed, showed several groups of bright lines, which cross the star-spectrum and extend out into the nebula-spectrum on each side. This is just what should happen if these "stars" in the nebula are balls of the nebulous matter condensed to a certain degree, but not quite to the state of ordinary stars, which show no bright lines in their spectrum.

As our readers doubtless remember, Mr. Huggins was the first to observe visually the spectrum of a nebula (in 1864), and the first to get a photographic impression of such a spectrum (in 1881). His latest observations have been visual as well as photographic, and have been made specially to test Mr. Lockyer's attempted explanation of the origin of the principal bright lines in the nebula-spectrum. The brightest of these lines, when first discovered, was for a time, attributed to *nitrogen*; but it soon appeared that this identification is untenable. The principal lines of hydrogen are unquestionably present, but this bright green line, which is far more conspicuous than any of the others, has thus far remained a mystery. Very recently, Mr. Lockyer, in connection with his "meteoric hypothesis," (in which he attempts to explain most of the phenomena of the heavenly bodies by the assumption that they are all mere swarms of such meteoric stones as fall upon the earth now and then), has

maintained that this line in the spectrum of the nebula is simply the remnant of one of the *bands* in the spectrum of *magnesium*—a metal which is commonly found in aerolites. The elaborate investigation of Dr. and Mrs. Huggins, who worked together in the observations, is conclusive in proving that this is an error; the mystery remains unsolved.

We must not close our sketch of recent progress without referring to Professor Holden's telescopic observations at the Lick Observatory. In the case of the numerous nebulæ which are not, like the majority, mere oval balls of shining cloud, but are irregular in form, and composed of numerous filaments surrounded by an envelope of fainter light, he finds that the apparent shape of the filament which forms the core of the structure can be explained by supposing it to be in reality a sort of "helicoid" or cork-screw spiral, seen under a special angle. A wire bent into such a helicoid, and looked at from different points of view, presents a great variety of outlines, which very accurately represent the otherwise inexplicable forms of these irregular nebulæ.

As is obvious from what has been said, the main conclusions which had been arrived at sixteen years ago are all confirmed. It is clearer than ever that the nebulæ are not "distant galaxies" far beyond the stars. They are not clusters of full-sized stars, which appear nebulous only because of their inconceivable remoteness, but they are *clouds*, and their luminosity is due to shining gases, among which hydrogen is present, though the gas which furnishes the brightest light remains still unknown. Moreover, these clouds, whatever else they may contain besides the luminous gases, are somehow kindred to the stars, and associated with them in such a way as to suggest almost irresistibly that the nebula is the material out of which stars are made.

The question of their permanence remains unsettled. It is altogether probable that processes are going on within them which slowly alter their conditions, but time alone can determine the rate and nature of such changes; the evidence, as yet, is indecisive.—*Popular Science News.*

PRINCETON, N. J., Oct. 31, 1889.

SCIENCE AT THE EIFFEL TOWER.

The great tower, 1,000 feet high, proves to have been one of the most successful features of the Paris Fxposition. The whole of the money paid out for its erection has been re-

turned to the share-holders, and the directors have in hand a clear surplus of about $300,000 after paying all the costs attending its exhibition. As a mere curiosity it will probably pay current expenses for many years to come, and be a boon to the poor of the city, as one-tenth part of the receipts are turned over to the poor fund. Now it is looming up as a valuable aid to scientific investigation, with a possibility that its contributions in that direction will compare favorably with its pecuniary success. A series of meteorological observations, extended through a period of 101 days, shows that the velocity of the wind averages fully three times as great at the top of the tower as at a station situated 1,650 feet away and 69 feet above the level of its base. No great storm occurred during the period of observation, so that the maximum movement is not yet ascertained, but it is found that at the top there is always a fairly strong breeze, the average daily velocity of which is 23 to 33 feet per second. It may be inferred from this that the air at a great distance above the surface of the earth is continually in rapid motion, and that the stillness often experienced lower down is due to friction of the air waves against land or water, which has always a retarding effect. The influence of these superior movements in diffusing and distributing noxious gases and vapors that have more gradually ascended from the surface must be of inestimable value, as undoubtedly to it is due the comparative freedom of man from contagion in spite of his general disregard of sanitary conditions to an extent which would otherwise be suicidal. It is also legitimate to infer that this air movement is a protection against injury from without by scattering and thus rendering innocuous the many masses of gaseous and meteoric matter which the scientists tell us enter the earth's atmosphere each year from the regions of inter-planetary space.

The anemometer is far from being the only instrument that has been made to take observations at the top of the Eiffel Tower. Automatic registers of varying electrical intensity are already being compared, with the promise of valuable results, and the spectroscope has told something of the difference in the apparent quality of the solar light, which is caused by the passage through those atmospheric strata, that are laden with dust and water vapor taken up from the surface. It is probable that even more attention will be paid to these and kindred researches now that the rush of Exposition visitors is over, and it is not too much to hope that the consequence will be a material extension of our knowledge of the physics of our own globe, and perhaps of those of some other worlds.—*Chicago Tribune* (Dec. 18, 1889).

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CURRENT CELESTIAL PHENOMENA.

THE PLANETS.

Mercury will be at greatest elongation east from the sun, 18° 51′, on the evening of Jan. 13, setting then an hour and thirty-three minutes later than the sun. For a few days it will be visible to the naked eye after sunset, near the southwest point of the horizon. There are no bright stars in that part of the sky, so that if, between 5 and 6 P. M. from Jan. 10 to 16, one sees a bright star near the southwest horizon he may be pretty sure that it is the planet Mercury. Later in the month Mercury will recede towards the sun, coming to inferior conjunction Jan. 29, and toward the middle of February will become visible in the morning.

Venus is "morning star," but not in favorable position for observation. She is passing around the sun and will be at superior conjunction Feb. 18.

Mars is coming into better position for morning observation. He passes, this month, out of the constellation Virgo into Libra, the Balance. Jan. 24 Mars will be 1° north of the bright star Alpha Libræ, and a little later will be in line with Alpha and Beta between the two. Mars will be the brightest of the three objects and may also be distinguished by his ruddy color. Mars will be at quadrature, *i. e.* 90° from the sun, Feb. 9. He is approaching the earth rapidly, and at the coming opposition, June 4, will be nearer than he has been since 1879. Every opportunity should be utilized to study the "canals," and this study should be begun as early as possible, in order to detect possible changes with the seasons of Mars. In the December number of *L'Astronomie* Ferdinand Meisel, an astronomer at Halle, attempts to explain the duplication of the canals as an optical phenomenon. He regards the canals as possibly actual wide water courses, over which transparent vapors accumulate in the form of cylindrical or semi-cylindrical lenses, refracting the rays which come to us, so that under certain conditions one, two, and even three separate images of the same canal may reach the earth.

Jupiter will be in conjunction with the sun Jan. 9, so that no observations can be obtained this month. At the last meeting of the Royal Astronomical Society considerable time was occupied in discussing observations of the dark shadow upon Jupiter at the time of its occultation by the moon Aug. 7 last. Opinion seemed to be about equally divided as to the cause of the shadow, some regarding it as the effect of contrast between the bright edge of the moon and the fainter light of Jupiter's disk, others believing it to be due to the secondary spectrum fringe around the image of the moon. In this connection Mr. E. L. Trouvelot publishes in *L'Astronomie* for December, 1889, an observation made by himself Aug. 6, 1876, of an occultation of Saturn by the moon, which, as he says, "does not in any way confirm the phenomenon of the dark vaporous line extending along the edge of the moon, noticed by several observers during the last occultation of Jupiter."

Saturn is moving slowly through Leo, which constellation may be seen in the east in the evening. Saturn is the bright yellow star with steady light just below the familiar group of the Sickle, quite near to the first

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magnitude star Regulus. In the June, 1889, number of the *Monthly Notices*, page 427, Mr. A. Marth called attention to an occultation of the satellite Japetus by the rings and globe of Saturn, beginning Nov. 1, 9.4*h*, and ending Nov. 2, 4.7*h* Greenwich mean time. In *Ciel et Terre*, Nov. 16, 1889, page 432, Dr. Terbe gives the following observations:

"Equatorial of 8 inches by Grubb; magnifying power, 132.

"Nov. 1, 3*h* 36*m* to 5*h* 03*m* A. M.; image bad, agitated. *Japetus is perfectly visible;* I see also Titan, Rhea, and Dione; but I can not discover Tethys.

"Nov. 2 the state of the sky prevented observation before 5*h* 59*m* A. M.; from 5*h* 59*m* to 6*h* 24*m* sky splendid; image of irreproachable sharpness; I see constantly Titan, Rhea, *Dione* and *Tethys; I cannot discern the least trace of Japetus.*

"Nov. 3, 4*h* 24*m* to 4*h* 33*m* A. M.; image bad; clouds interrupt further observations. I see Titan, *Japetus*, Rhea, Tethys and Dione; these last four satellites are very near to Saturn, Japetus and Rhea below the west ansa of the rings; Tethys and Dione near the point of the east ansa."

Uranus will be in quadrature with the sun Jan. 16, and is in favorable position for observation in the morning. He may be found in the constellation Virgo about half way between Spica and the fourth magnitude star Kappa. There are several stars of about the same brightness in the vicinity, but with a telescope Uranus may be distinguished from the stars by his disk and dull green color.

Neptune may be observed during the whole night. He will be in conjunction with the moon Jan. 29, at noon. At 8 o'clock P. M., on that date, he will be about 4° or 8 diameters of the moon directly west of the latter.

Planetoids. We have been requested to give occasionally the places of the brighter planetoids. We shall be glad to comply with the request. During the coming month none of the bright planetoids will be in position for observation.

Sunspots have not been so numerous during the past three months as during the summer. On Dec. 20, however, two groups of spots and a large group of brilliant faculæ were observed at Northfield. At the last meeting of the Royal Astronomical Society (*Observatory* Vol. XII, p. 425), Father Perry, of Stonyhurst College Observatory, England, read a "Note on Solar Spots." "He said he thought he might as well call attention to the spots in the southern hemisphere which had been observed a long way from the equator. They all knew that as the minimum of sunspots was coming on a certain number of spots showed themselves in high latitudes. At present the spots were entirely confined to the southern hemisphere, and at the end of last year one spot was seen in high southern latitudes, and they were at present increasing.

"On June 5th there was one in 29° south latitude. On 30 June there was a remarkable spot in 40° south latitude. They were seldom seen so far from the equator as that. That spot was observed in America, and he dared say it was observed at Greenwich, but he had only heard of it being observed at Stonyhurst and in America. On Aug. 2d there was a group of spots and another on Sept. 7th, but they were not in very high latitude; they were only 21°.3 and 22°; on Oct. 8th 28½°, and Oct. 10th 25°; so

that the number of spots was increasing in these high latitudes; and though he thought the minimum might not yet have arrived, because these spots generally showed themselves before the minimum, he thought they could not be far from it. The one at 40° was among the brightest on record. He did not think they could conclude that though those remarkable ones did follow the minimum there was such a variation in the period of eleven years that they could not be at all certain about the matter, but it seemed at present as if the spots were on the increase."

Mr. Maunder, of the Royal Observatory at Greenwich, confirmed Father Perry's observations of spots in high latitudes from the Greenwich daily photographs of the sun and said: "The general run of the recent phenomena of sunspots has given me the decided impression that the minimum is already past. A fine group came on the sun on June 16 last, and re-appeared in the two following rotations. Then a fine group appeared on Aug. 3 in southern latitude 22°, and this also has been seen in the two following rotations. Comparing the general aspect of the sun with that of 1879, the indications it presents now are similar to those after the former minimum was definitely passed. It is true that if the minimum be already passed we have not had so flat a trough for the solar wave as in 1878 and 1879, for we had then five months in which there were practically no spots at all. But if we take the mean daily spotted area of the sun for the first half of this year, stopping short at what seems to have been the commencement of the revival (the appearance of the group on June 16) we shall find it smaller than at the minimum in 1888. In 1878 the mean daily spotted area was 24 millionths of the sun's visible hemisphere; in 1888, 89 millionths; but for the half year ending June 15, 1889, it was only 18 millionths."

Professor Dr. Spoerer, of Potsdam, also calls attention to the same subject in the *Astronomische Nachrichten* No. 2936.

1889.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			MERCURY.		
Jan. 25	21 03.5	−13 56	7 36 A.M.	12 43.6 P.M.	5 51 P.M.
Feb. 4	20 18.9	−16 04	6 22 "	11 19.7 A.M.	4 18 "
14	20 13.9	−18 10	5 47 "	10 35.3 "	3 24 "
			VENUS.		
Jan. 25	20 09.7	−21 00	7 14 A.M.	11 49.8 A.M.	4 25 P.M.
Feb. 4	21 01.7	−18 08	7 14 "	12 02.2 P.M.	4 51 "
14	21 51.6	−14 24	7 07 "	12 12.6 "	5 18 "
			MARS.		
Jan. 25	14 45.3	−14 31	1 22 A.M.	6 26.3 A.M.	11 31 P.M.
Feb. 4	15 05.7	−16 02	1 09 "	6 07.2 "	11 05 A.M.
14	15 25.3	−17 22	12 55 "	5 47.5 "	10 40 A.M.
			JUPITER.		
Jan. 25	19 42.0	−21 37	6 50 A.M.	11 22.2 A.M.	3 55 P.M.
Feb. 4	19 51.7	−21 13	6 18 "	10 52.5 "	3 27 "
14	20 01.2	−20 48	5 46 "	10 22.7 "	2 59 "
			SATURN.		
Jan. 25	10 19.6	+12 12	7 05 P.M.	1 57.4 A.M.	8 50 A.M.
Feb. 4	10 16.9	+12 29	6 22 "	1 15.3 "	8 09 "
14	10 13.9	+12 47	5 38 "	12 33.0 "	7 28 "

URANUS.

	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Jan. 25	13 40.0	− 9 45	11 52 P.M.	5 17.1 A.M.	10 42 A.M.
Feb. 4	13 39.9	− 9 44	11 13 "	4 37.7 "	10 02 "
14	13 39.6	− 9 42	10 33 "	3 58.1 "	9 23 A.M.

NEPTUNE.

Jan. 25	4 00.3	+18 54	12 17 P.M.	7 39.0 P.M.	3 01 A.M.
Feb. 4	4 00.0	+18 54	11 37 "	6 59.4 "	2 22 "
14	4 00.0	+18 55	10 58 "	6 20.1 "	1 42 "

THE SUN.

Jan. 25	20 32.7	−18 50	7 26 A.M.	12 12.7 P.M.	4 59 P.M.
Feb. 4	21 13.5	−16 04	7 16 "	12 14.2 "	5 12 "
14	21 53.2	−12 51	7 02 "	12 14.4 "	5 27 "

Occultations Visible at Washington.

Date.	Star's Name.	Magnitude.	IMMERSION. Wash. Mean T. h m	IMMERSION. Angle f'm N. P't. °	EMERSION. Wash. Mean T. h m	EMERSION. Angle f'm N. P't. °	Duration. h m
Jan. 24	B. A. C. 17	6	4 54	86	5 58	207	1 04
29	ε Tauri	3½	12 43	83	13 46	262	1 03
31	3 Geminorum	6½	12 23	53	13 22	312	1 00
31	6 Geminorum	6½	13 49	111	14 52	267	1 04
Feb. 2	84 Geminorum	6½	12 29	182	12 47	205	0 18
9	80 Virginis	6	15 23	114	16 45	317	1 22
12	ν^2 Scorpii	4½	16 12	143	17 19	270	1 07

Elongations and Conjunctions of Saturn's Satellites.

[Central time; E = east elongation; W = west elongation; I = inferior conjunction (north of planet); S = superior conjunction (south of planet.)

JAPETUS.

Jan. 22, S. Feb. 10, E.

TITAN.

Jan. 19	5.0 P. M.	I.	Jan. 31	3.2 P.M.	E.	Feb. 12	1.0 P. M.	S.
23	4.4 P. M.	W.	Feb. 4	2.5 P.M.	I.			
27	3.8 P. M.	S.	8	1.7 P.M.	W.			

RHEA.

Jan. 17	12.3 P.M.	E.	Jan. 31	1.2 A.M.	E.	Feb. 13	2.1 P. M.	E.
22	12.7 A.M.	E.	Feb. 4	1.4 P.M.	E.			
26	1.0 P.M.	E.	9	1.9 A.M.	E.			

DIONE.

Jan. 18	1.8 A.M.	E.	Jan. 29	12.4 A.M.	E.	Feb. 8	10.8 P.M.	E.
20	7.4 P.M.	E.	31	5.9 P.M.	E.	11	4.4 P.M.	E.
23	1.1 P.M.	E.	Feb. 3	11.6 A.M.	E.	14	10.1 A.M.	E.
26	6.7 A.M.	E.	6	5.1 A.M.	E.			

TETHYS.

Jan. 17	4.6 P.M.	E.	Jan. 29	12.3 A.M.	E.	Feb. 9	9.9 A.M.	E.
19	1.8 P.M.	E.	30	9.5 P.M.	E.	11	5.2 A.M.	E.
21	11.1 A.M.	E.	Feb. 1	6.8 P.M.	E.	13	2.5 A.M.	E.
23	8.5 A.M.	E.	3	4.0 P.M.	E.	14	11.8 P.M.	E.
25	5.7 A.M.	E.	5	1.3 P.M.	E.			
27	3.0 A.M.	E.	7	10.6 A.M.	E.			

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Phases of the Moon.

			Central Time. d	h	m	
New Moon	1890	Jan.	20	5	49	P. M.
First Quarter	"	"	27	2	16	P. M.
Full Moon	"	Feb.	4	7	14	P. M.
Last Quarter	"	"	12	12	51	P. M.
Perigee	"	Jan.	20	9	00	A. M.
Apogee	"	Feb.	2	8	00	A. M.

Periodic Comets to Return in 1890. Mr. W. T. Lynn in *Knowledge*, Dec. 2, 1889, p. 33, and Mr. W. E. Plummer in *The Observatory*, December, 1889, p. 432, give brief notices of the periodic comets whose return to perihelion is to be expected in 1890. There are four, possibly five of them, all telescopic and of short period, belonging to the Jupiter family of comets. Of these, two, Brorsen's and D'Arrest's, have been observed at several apparitions, but both escaped detection at the last return. The other three have been seen only at one apparition, so that their periods are somewhat doubtful.

Barnard's Comet 1884 II was discovered July 16, 1884, by E. E. Barnard, at Nashville, Tenn. The period assigned by Dr. Berberich is 5.36 years, so that this comet should be now very near perihelion. The elements of its orbit bear a close resemblance to those of the lost De Vico comet, 1844 I, and also to those of Finlay's comet 1886 VII. They cannot be identical, but probably belong to a system of faint comets which escape observation except when their perihelia are favorably situated with reference to the earth.

Brorsen's Comet was discovered at Kiel, Feb.26, 1846. Its period is very nearly five and one-half years, so that its apparitions occur alternately in the spring and autumn. It was seen in 1857, 1868, 1873, and 1879, but escaped observation in 1884. It is due at perihelion this year about February 25, its course passing through the constellations Pisces, Andromeda, and Cassiopeia. It should not escape detection this time. In another place Mr. G. A. Hill gives a sweeping ephemeris which will aid observers in their search for the faint visitor.

Mr. Plummer says: "The orbit is a very interesting one, for it approaches very closely to that of Jupiter and at intervals of ninety-five years the comet encounters the planet, and a very decided change in the orbit ensues. This close approach last occurred in 1842 May, when the comet's motion, which had been previously hyperbolic was changed into the elliptical form in which the comet now moves. In 1937 this proximity will again take place, when the orbit will probably resume its hyperbolic curve, and be lost to the solar system."

Coggia's Comet, discovered Nov. 10, 1873, was thought, from the investigation of Professor Weiss, to be identical with one detected Feb. 23, 1818, by Pons, but, although the derived period is about five and one-half years, it has not been seen since 1873. If this period is correct the comet is due early in 1890. We have no ephemeris at hand.

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Denning's Comet 1881 V, discovered at Bristol Oct 4, 1881, has been thought to be identical with that discovered by Blanpain Nov. 28, 1819, for which Encke derived a period of 4.8 years. The most reliable orbit of Denning's comet, however, gives a period of 8.6874 years. This period, computed by Dr. Boy Matthiesen (A. N. 121, p. 359), has a probable error of only four days, so that the comet should come to perihelion in May, 1890, but with a theoretical brightness of only one-third that which it had when discovered in 1881. Also the comet in 1887 approached very near to Jupiter, so that its orbit may have suffered considerable perturbations, which must be calculated approximately before a sweeping ephemeris can be computed.

D'Arrest's Comet was discovered at Leipzig June 27, 1851; it has a period of six and a half years and was seen in 1857, 1870 and 1877 but was not seen at its last return in the winter of 1883. According to Winnecke this is the faintest of the periodic comets, but its course this year will be similar to that which it followed in 1883, when it was a fairly conspicuous object in the telescope. This comet is due at perihelion about the third week in September. An accurate ephemeris will doubtless be soon published. Such ephemerides have hitherto been issued by M. Leveau, of Paris.

Comets of 1887-8.

Comet 1886	VIII	1887 *c*	Barnard, Jan. 23.
Comet 1887	I	1887 *a*	Thome, Jan. 18.
Comet 1887	II	1887 *b*	Brooks, Jan. 23.
Comet 1887	III	1887 *d*	Barnard, Feb. 16.
Comet 1887	IV	1887 *e*	Barnard, May 12.
Comet 1887	V	1887 *f*	Brooks, Aug. 24...Olbers's.
Comet 1888	I	1888 *a*	Sawerthal, Feb, 18.
Comet 1888	II	1888 *b*	Encke's Comet.
Comet 1888	III	1888 *c*	Brooks, Aug. 7.
Comet 1888	IV	1888 *d*	Faye's Comet.
Comet 1888	V	1888 *f*	Barnard, Oct 30.
Comet 1889	I	1888 *e*	Barnard, Sept. 2.

O. C. WENDELL.

Harvard College Observatory, Dec. 6.

The Return of Brorsen's Comet. This comet is now almost due. In the *Astronomische Nachrichten*, No. 2220. Dr. Schulze, of Dobelu has made an extended investigation of the orbit of this comet, and from his elements for 1879, brought up to 1890, I have computed the following sweeping ephemeris. The period of the comet is becoming less, as will be seen by the following intervals as deduced from the five observed returns.

1846, 2034.1 days.
1857, 2022.7 days.
1868, 2002.4 days.
1873, 1999.4 days.
1879, 1994.9 days.

As the comet was missed in 1884 I have used 3929 days as the date of the next perihelion passage or twice that as shown by Dr. Schulze's elements for 1879, namely 1994.9 days. I then decreased the period by 12 days and computed another ephemeris. The two will be found below.

These two values give as the dates of perihelion passage Feb. 17 and March 1, 1890, respectively. Dr. Lamp, of Kiel, has an ephemeris for this comet in a late number of the *Nachrichten*. He has made the date of the next perihelion passage on Feb. 24, 1890, which is intermediate to those selected by me.

GEO. A. HILL.

Naval Observatory, Washington, D. C., Dec. 19, 1889.

Sweeping Ephemeris for Brorsen's Comet.

Perihelion Passage March 1, 1890.

DATE.	α			δ		Δ
	h	m	s	°	′	
Dec. 31	22	55	30	— 32	45.4	0.17165
Jan. 1		56	39	32	23.5	
2		57	48	32	1.5	
3		58	57	31	39.5	
4	23	0	7	31	17.5	0.16650
5		2	44	30	51.4	
6		5	21	30	25.3	
7		7	58	29	59.2	
8		10	35	29	33.0	0.15245
9		12	35	29	6.4	
10		14	36	28	39.8	
11		16	37	28	13.2	
12		18	38	27	46.6	0.14122
13		20	43	27	18.0	
14		22	49	26	49.4	
15		24	55	26	20.8	
16		27	1	25	52.1	0.12866
17		29	46	25	18.5	
18		32	32	24	44.9	
19		35	7	24	11.3	
20		37	43	23	37.7	0.11044
21		38	28	23	7.5	
22		39	13	22	37.2	
23		40	58	21	7.0	
24		42	43	21	36.7	0.09623
25		45	28	20	59.4	
26		47	13	20	22.1	
27		50	28	19	44.8	
28		53	44	19	7.5	0.08153
29		56	2	18	28.2	
30	23	58	20	17	48.9	
31	0	0	38	17	9.6	
Feb. 1		2	57	16	30.3	0.06069
2		5	15	15	45.3	
3		7	34	15	0.3	
4		9	53	14	15.3	
5		12	12	13	30.2	0.04087
6		14	28	12	41.3	
7		16	44	11	52.3	
8		19	0	11	3.3	
9		21	17	10	14.3	0.01713
10		23	28	9	20.1	
11		25	39	8	25.9	
12		27	55	7	31.7	
13		30	2	6	37.5	
14		32	7	5	32.6	9.99198
15		34	12	4	27.7	
16		36	17	3	22.8	
17	0	38	22	— 2	17.9	9.95791

Perihelion Passage Feb. 17, 1890.

DATE.	α			δ		Δ
	h	m	s	°	′	
Dec. 31	23	0	55	— 32	47.3	0.09176
Jan. 1		2	18	32	18.7	
2		3	22	31	50.1	
3		4	36	31	21.5	
4		5	50	30	52.9	0.07928
5		8	11	30	48.3	
6		10	33	29	49.6	
7		12	55	29	18.0	
8		15	17	28	46.3	0.05917
9		16	0	28	16.5	
10		16	44	27	46.7	
11		17	28	27	16.9	
12		18	12	26	47.1	0.04483
13		20	41	26	5.6	
14		23	10	25	34.8	
15		25	39	24	53.3	
16		28	8	24	21.0	0.02843
17		30	0	23	41.8	
18		31	52	23	2.8	
19		33	44	22	23.4	
20		35	37	21	44.2	0.00709
21		37	25	21	0.8	
22		39	13	20	17.4	
23		41	1	19	34.0	
24		42	49	18	50.6	9.98321
25		44	29	18	2.0	
26		46	10	17	13.3	
27		47	51	16	24.7	
28		49	32	15	35.9	9.95661
29		51	0	14	41.6	
30		52	28	13	47.2	
31		53	56	12	52.9	
Feb. 1		55	24	11	58.5	9.92693
2		56	8	10	52.9	
3		57	32	9	47.3	
4		58	36	8	41.7	
5	23	59	41	— 7	36.0	9.88888

Comet d 1889 (Brooks, July 6), was observed on six nights from Nov. 14 to Nov. 26, by Professor Young and his assistant, Mr. Miller, with the 23-inch equatorial, at Princeton. "On Nov. 14 both observers saw the companion comet, about 5′ from the principal comet, at a position angle of between 50° and 60°. It was still better seen on the 15th by Mr. Miller but not quite well enough to admit of observation on the bars. The main comet was decidedly elongated by a short brush 1½′ in length directed nearly toward the companion." The best elements of this comet are probably those given below, computed by Dr. O. Knopf, (A. N. No. 2936)

whence the period is 7.071 years. In *Astronomical Journal*, No. 204, Mr. S. C. Chandler has published very similar elements with a period of 7.039 years. He calls attention to the very close approach of the comet to Jupiter in 1886 when for three months the distance between them did not exceed 0.100 of the earth's distance from the sun. It is probable that the character of the orbit was radically changed in that time.

ELEMENTS.

$T =$ 1889 Sept. 29.7436 Berlin M. T.
$\omega =$ 343°18′ 56.5″
$\Omega =$ 17 58 29.6
$i =$ 6 3 59.6
Mean Equinox 1889.0
$\varphi =$ 28 4 13.3
$\mu =$ 501″.8156
$U =$ 7.071 years.

EPHEMERIS.

1889.	α	δ	log r	log Δ	L.	1889.	α	δ	log r	log Δ	L.
	h m s	° ′					h m s	° ′			
Jan. 4	0 45 54	+ 7 52.6	0.3243	0.2752	0.6	Jan. 27	1 23 18	+12 10.8			
5	47 26	8 3.8				28	25 1	12 21.9	0.3406	0.3472	0.4
6	48 58	8 15.1				29	26 44	12 33.0			
7	50 31	8 26.3				30	28 28	12 44.0			
8	52 5	8 37.6	0.3269	0.2878	0.5	31	30 12	12 55.0			
9	53 39	8 48.8				Feb. 1	31 57	13 6.0	0.3435	0.3583	0.4
10	55 14	9 0.1				2	33 42	13 17.0			
11	56 49	9 11.4				3	35 27	13 27.9			
12	0 58 25	9 22.7	0.3295	0.3002	0.5	4	37 12	13 38.8			
13	1 0 1	9 34.0				5	38 58	13 49.6	0.3464	0.3691	0.3
14	1 38	9 45.3				6	40 44	14 0.4			
15	3 15	9 56.6				7	42 31	14 11.2			
16	4 53	10 7.8	0.3322	0.3123	0.5	8	44 18	14 21.9			
17	6 31	10 19.1				9	46 5	14 32.6	0.3493	0.3797	0.3
18	8 10	10 30.3				10	47 52	14 43.3			
19	9 49	10 41.5				11	49 40	14 53 9			
20	11 29	10 52.7	0.3350	0.3242	0.4	12	51 28	15 4.5			
21	13 9	11 3.9				13	53 16	15 15.0	0.3523	0.3900	0.3
22	14 50	11 15.1				14	55 4	15 25.5			
23	16 31	11 26.3				15	56 53	15 36.0			
24	1 18 12	11 37.4	0.3378	0.3358	0.4	16	1 58 42	15 46.4			
25	1 19 54	11 48.6				17	2 0 32	+15 56.8	0.3553	0.4000	.03
26	21 36	+11 59.7									

Comet f 1889 (Swift, Nov. 17) was observed at Lick Observatory Nov. 20, 21, 22, 25 and 26.

New Comet g 1889 (Borrelly, Dec. 12). A telegram, Dec. 15, announces the discovery of a faint comet by Borrelly at Marseilles. Position, Dec. 12.311 Gr. M. T., R. A. 18*h* 07*m* 00*s*; Decl. + 48° 53′; daily motion, + 1*m* 12*s*; south 1°. *Astronomical Journal*, No. 207, has an observation by Barnard Dec. 15.6425 Gr. M. T., R. A. 18*h* 0*m* 0.115*s*; Decl. + 45° 30′ 10″.

The Hartford Fire-Ball. In the Hartford *Courant* of Dec. 12, 1889, an interesting, though somewhat indefinite, account is given of the fall of a fire-ball, the fragments of which seem to indicate that its origin was meteoric. The fall was observed on Tuesday evening, Dec. 10, at 8*h* 15*m*, in the midst of Seyms street, opposite the residence of Mr. Charles B. Andrus. So far as now known it was only seen by a lad twelve years of age, who claims that he saw "a big ball of fire fall in the middle of the street." By those who looked into the matter it is thought that the object came from the northwest quarter of the heavens, at an angle of elevation of thirty or forty degrees. At the place of its fall fragments were found, and the

ground for a foot and a half square was colored a greyish white shade by the fine ashes which apparently were thrust into the soil that formed a thin covering of a road made of crushed stone. There were no large fragments, and the appearance of those most carefully examined led to the belief that the object was a meteorite.

Brilliant Meteor. December 17, at 4*h* 45*m* P. M., I witnessed a most remarkable meteoric flight. Pardon the full details as I wish the occurrence to be correctly stated for the purpose of comparison with other possible observations of the same object. I went to my Observatory to make some repairs and as I reached the front of it, and facing east the meteor flashed into view. As may be judged by the time it was still strong daylight, but so bright was the meteor that it attracted my attention immediately. It was fully as bright as Venus would appear in a dark sky, and at the most favorable position in her orbit for greatest brightness. What struck me most was the decided green color of the meteoric light. The color to me was intense. Following the meteor was four or five attendant but lesser objects, which seemed to be of an orange color. I give below a general view of the transit, and wish to call attention to the rays emanating eastward from the prim-ary, and which seemed remarkable on account of the strong sunlight present.

The position of these rays, as above, is about as I saw them, and may indicate more than ordinary atmospheric appearances. Unfortunately the sky was not wholly free from clouds and haze, and the termination of the passage may be in error. I extract from the notes the following positions to which I may add that they were obtained by the circles of the telescope after careful reference to the position of objects relative to the witnessed path:

Apparition in δ $= +21°$ nearly.
Extinction in δ $= -9°$ "
Altitude by hour circle $= 15°$ "

WILLIAM EDWARD WOOD.

Washington, D. C.

United States Scientific Expedition to West Africa. We have received Bulletins Nos. 3 and 5 of the United States Scientific Expedition to West Africa under the direction of Professor D. P. Todd, of Amherst College. These respectively relate to "Fish of the Congo Basin" and "Bibliography of Ki-mbundu."

On Dec. 23 news came *via* London from St. Paul de Loando to the effect that numerous photographs of the eclipse were obtained by the American Expedition during the period of totality. It is reported that clouds gave some trouble, but the apparatus in use worked perfectly. The photographs made on the ship Pensacola which stood far out at sea are likely to be particularly useful. It is also reported that the English observers at St. Paul de Loando obtained no good observations on account of bad weather. We hope for better reports later.

XUM

NEWS AND NOTES.

We have found it difficult during the last two months to secure, within easy reach, the quality of engraving desired for the illustration of some articles already in hand. After three unsuccessful trials by artists in St. Paul and Chicago, supposed by us to be among the best, we have deferred one biographical sketch until next month. We are still trying with hope of success.

The beautiful frontispiece picture of the great nebula of Andromeda is a copy from the last issue of *Himmel und Erde*. It is presented at this time to accompany the article entitled "News from the Nebulæ," by Professor Young and elsewhere given in this number. If the reader compares the cut given below, with the fine photograph by Roberts of England, of which our frontispiece is a close process copy he will find it difficult to believe that the two pictures represent the same object. This one gives the the appearance

of the nebula as seen in Herschel's telescope and as pictured in some excellent text-books on astronomy now in use. The later drawing by Trouvelot shows the two nearly parallel canals, so-called, observed by Mr. Bond at Cambridge, some years ago by the aid of the 15-inch refractor. We now know the shape and extent of these canals and the general features of this grand nebula, which is one of the late noble triumphs of celestial photography. But, surely, this is only the beginning of wonders, as we believe from that astonishing article by professor W. H. Pickering, the leader of this number. We supposed we knew something about old Orion, but his "bands" are apparently infinitely more than those that Job saw.

Wm. M. Pierson, vice-president of the Astronomical Society of the Pacific, has recently set up an 8½-inch reflecting telescope in his private Observatory in San Francisco. The instrument was made by J. A. Brashear, of Alleghany City, Pa., is equatorially mounted with driving clock and circles, and has proved very satisfactory to its owner.

Astronomical Society of the Pacific. By the kindness of Mr. Charles Burckhalter, secretary of the Astronomical Society of the Pacific, and of members of the staff of Lick Observatory, we are able to give the results of much valuable recent work for astronomy from the western coast. It is a great pleasure to us to notice the general interest in our science that has so quickly sprung up on the other side of the Rockies. But it is more wonderful still to know the kind of astronomical work this young society is doing. It is certainly work that matches well the best of its kind we know of. The officers have also done nobly, as we think, to enlist a general public interest in what they are doing, and the consequence is quick and generous aid, in the way of donations, as illustrated by Mr. Montgomery's handsome gift of $2,500. The society is congratulated on its unusual success in so many ways.

Annual Report of Harvard College Observatory. The forty-fourth annual report of the Director of the Astronomical Observatory, of Harvard College, was presented to the Visiting Committee Dec. 14, 1889. This report speaks of the death of Mr. J. I. Bowditch, an old and active friend of the Observatory, the Bruce gift of $50,000, the continued aid of Mrs. Draper and that of the Boyden Fund for the Peru expedition, which enables the Observatory to extend its important investigations to include the southern stars. A second expedition to Southern California furnishes the Observatory a mountain station, under climatic conditions superior to those of the eastern portion of the United States.

The east equatorial has been mainly used by Mr. Wendell, and it has six new eye-pieces and two square bar micrometers made of tinfoil mounted on glass by Professor Rogers. This instrument has been busy, evidently, from the range of work done by it. The Meridian Circle has been used in observing the southern zone, comprising the declination from $-9^\circ\ 50'$ to $-14^\circ\ 10'$, by Professor Searle and Mr. Dunne. The total number of observations is 8565: 287 relating to circumpolar stars, 1,024 to fundamental stars, 6889 to zone stars, and 365 to stars incidentally observed. Reductions of the Meridian Circle observations are being forwarded as rapldiy as the limited force of computers can do the work. The twelve-inch horizontal telescope has been used by Mr. G. E. Hale in an investigation of the solar spectrum.

In the latter part of this report, fuller mention is found of the uses made of the Boyden fund, the Bruce Photographic Telescope, now in process of construction, which is to be 24-inches clear aperture, with focal length of eleven feet, and its contemplated work; the library, the time service, telegraphic announcements and publications for the year, all of which indicate an excellent showing for the work of this Observatory.

Professor A. E. Haynes, Department of Mathematics and Astronomy, Hillsdale College, Michigan, some time ago called our attention to an observation of the Crater Ptolemais, made Dec. 10, 1888, at 8 o'clock P. M., Standard Central time. He saw a part of the bottom of the Crater Ptolemais lighted up by reflection from the lighted edge. He has not before nor since observed the same phenomenon.

XUM

Notes from the Advance Sheets of the Publications of the Astronomical Society of the Pacific.

Great Telescope for Los Angeles. Authentic information regarding the proposed forty-inch refractor for Wilson's Peak is difficult to obtain. A newspaper report of an interview with Mr. A. G. Clark on September 28, recites that one of the discs (now on exhibition at Paris) probably arrived in Boston in October. The other disc is not yet cast, and M. Mantois is, apparently, not willing to undertake the work without a contract, which is not yet executed. The trustees of the Fund have, so it is said, authorized Mr. Clark to pay $10,000 for two satisfactory forty-inch discs, which is not an unreasonable price by any means. Mr. Clark offered to make the objective and the mounting for $100,000, during his visit to California in the winter of 1888-9. So far as is now known, the fund available for the telescope does not yet exceed $150,000. Probably $300,000 to $400,000 would build and equip the Observatory. E. S. H.

Force of Gravity at Mt. Hamilton and San Francisco. Mr. E. D. Preston of the U. S. Coast and Geodetic Survey has published his report on gravity determinations in the Pacific Ocean (Bulletin No. 11, U. S. C. and G. S., 1889). The force of gravity at Washington being 1.000000, that at San Francisco (Professor Davidson's Observatory) is 0.999854 and at the Lick Observatory it is 0.999544. Determinations of g at four stations in the Hawaiian Islands and for a station at Caroline Island are also given.

E. H. S.

Rainfall on Mount Hamilton. Meteorological observations have been kept at Mount Hamilton since 1880. The following table of rainfall on the summit is the best available summary. This rainfall is considerably more than that in the Santa Clara Valley near San Jose (about 13.4 inches) and it is probably considerably less than the fall in some of the canyons and valleys immediately surrounding the mountain. The great variations in the annual amount of rainfall are interesting from a meteorological point of view, and decidedly inconvenient from a practical one, especially as our reservoir capacity is not quite adequate. E. S. H.

Rainfall at Mount Hamilton in the years 1880–89.

Month.	1880-81	1881-82	1882-83	1883-84	1884-85	1885-86	1886-87	1887-88	1888-89
	in.	in.	in.	in.	in.	in.	in.	in.	in.
July	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.00
August	0.00	0.00	0.00	0.00	0.15	0.00	0.00	0.00	0.02
September	0.00	0.10	0.00	0.65	0.65	0.15	0.00	0.33	0.43
October	0.00	0.33	6.16	2.15	3.71	0.05	0.60	0.09	0.09
November*	0.50	0.91	3.45	1.48	0.01	—	2.82	0.90	3.27
December	9.68	9.72	1.93	2.05	33.84	—	2.34	11.25	4.23
January	3.51	3.55	3.10	5.60	1.99	—	2.83	10.04	1.04
February	5.99	2.90	3.75	12.76	0.57	1.80	7.80	1.38	1.42
March	1.13	5.40	8.66	16.35	1.15	5.77	1.39	3.40	6.17
April	0.98	4.70	2.66	11.96	2.08	6.79	5.75	0.68	1.92
May	0.09	0.48	7.55	1.24	0.16	0.70	0.25	1.25	3.21
June	0.33	1.06	0.00	3.85	0.36	0.00	0.30	0.67	0.00
Sums	22.21	29.15	37.26	58.09	44.67	0.00	24.08	30.03	21.80

* November, 1880—One shower, amount assumed to be 0.50 in.

Mean annual rainfall (8 years), July to July = 33.41 in.

Stability of the Great Equatorial. Observations for the position of the great telescope have been made by Messrs. Schaeberle and Keeler, as below:

1888, July 27, azimuth = + 36″; level = 8″ too low.
1889, May 18, " = —— " = 36″ " "
Sept. 16, " = + 83″ " = 58″ " "

There appears to be a slight progressive change in level and probably in azimnth. I.

Lick Observatory Photographs of the Moon. Knowledge for Oct. 1, 1889, contains an article by the editor (Mr. Raynard), on the Moon as seen in the Lick Telescope. Excellent reproductions of five silver prints made by the Direct Photo-Engraving Company of London, accompany the article. Mr. Raynard's remarks upon the temperature of the moon and upon the possibility of the existence of snow fields on its surface, are well worth close attention. E. H. S.

Mountain Observatories. Telescopes . . . . "cannot be formed so as to take away that confusion of rays which arises from the tremors of the atmosphere. The only remedy is a most serene and quiet air, such as may perhaps be found on the tops of the highest mountains above the grosser clouds."—Sir ISAAC NEWTON, in his *Opticks*, A. D. 1730.

Latitude by the Zenith Telescope. Continental astronomers have lately employed the zenith telescope in determining latitudes at many stations. With other instruments arranged for this method the results have been so favorable, that Wauschaff, of Berlin, has constructed a zenith telescope whose accuracy is remarkable; the probable error of observation upon a pair of stars for one night being but ± 0.″15, (fifteen hundredths of a second) at the station Schucekoppe of the Geodetic Institute of Berlin. The observer was Professor Albrecht; the instrument has an objective of Jena glass of 2½ inches in aperture, and two delicate levels for mutual control. T. H. S.

Repairs at Ann Arbor. The dome of the Observatory at Ann Arbor was put up some thirty-five years ago and has received little attention since. It moves on five cannon balls, and, in the course of years, the rotation of the dome has become more and more difficult, until it is now practically immovable. At their last meeting, the Regents of the University authorized its repair and the matter has been placed in the hands of Warner and Swazey, of Cleveland, who will put in their superior running apparatus. The work will probably not be completed before the first of next April. The same manufacturers will also put their shutter on the dome.

Annual Companion to the Observatory, 1890. This very useful publication for the year 1890 is promptly at hand. Although somewhat re-arranged and abridged, it still contains everything of importance to be expected in such a publication. The readers of THE MESSENGER who use information of this kind can not do better than to secure a copy of the Companion to the Observatory for the year 1890.

The Fauth Meridian Circle of Cincinnati Observatory. I am glad to respond to your request for some notes concerning the work of this Observatory, and especially with respect to the performance of the New Meridian Circle. The most satisfactory test of the excellence of an instrument is undoubtedly to be obtained from the discussion of an extended series of observations made with it. For such a test of our Meridian Circle we have now abundant material, for since the instrument was set up in August, 1888, over 3,000 observations have been taken. The greater part of these are of proper-motion stars, the re-determination of their movements, and the compilation of a catalogue of these interesting objects being the chief work upon which we are now engaged. The portion of this catalogue covering the first six hours of right ascension is already nearly completed. Before the remainder can be finished it will be necessary to await the appearance of the rest of the new Paris Catalogue, so that advantage may be taken of the many new cases of proper motion which the comparison of modern observation with Lalande's positions is sure to bring to light.

Besides the class of work referred to, we have taken part in the observations of the minor planets, Victoria and Sappho, and their comparison stars according to the programs published by Dr. Gill. Of the 42 stars in the first series, 186 observations were made, and 8 of the planet itself. Of the 43 stars in the second series, 244 observations were obtained, and 16 of the planet. Each star was observed in both positions of the circle, but a comparison of the results, west and east, shows no systematic differences either in right ascension ordeclination. The average probable error of a single observation as computed from the Victoria stars gave $\pm$ 0.056s in right ascension, and $\pm$ 0.58″ in declination; but several nights were employed when the definition was far from good. During the fall months when the second series was observed the average definition was somewhat better, and the resulting probable errors show a decided improvement, being $\pm$ 0.050s and $\pm$ 0.44″ respectively. I may mention that only one observation was rejected, and that was marked poor at the time. These values will compare favorably with those obtained by instruments of similar size, but the principal danger to be feared is not the accidental errors of observation so much as constant errors. If a star is always observed upon the same divisions of the circle, no matter how well the individual results agree, we can never be sure that we have the true declination. This danger can be avoided by shifting the circle at the end of each year and re-observing the same stars upon different divisions. This plan has been adopted here, but as yet no evidences of any constant error have been detected.

Another good test of the accuracy of the graduation is obtained from the agreement of the equator point as given by the observation of fundamental stars differing widely in declination. In our practice six such stars are usually observed on each night, one or two being circumpolar stars. The average probable error of the equator point from a single star comes about half a second. Into this, of course, enters the uncertainty of the star positions and of the refraction, as well as the errors of graduation. The stability of the instrument and the ease with which it is manipulated are very satisfactory. On the whole Messrs. Fauth and Saegmueller are to be congratulated on the excellence of their workmanship. J. G. PORTER,

Dec. 17, 1889. Director of Cincinnati Observatory.

A Wonderful Nebulous Ring. In about the year 1865 while comet-seeking with a 4½-inch telescope, I swept across an object which, from its size and peculiar appearance, I thought might be either a comet or, more likely, a glow from a cluster of bright stars closely adjoining it. Subsequently, finding no motion, I settled upon the latter theory, but, observing it several times during succeeding years, I came to notice that the supposed glow was all on one side of the cluster, and became convinced that it was a nebula, probably a well known one.

In 1880 I came into possession of Sir John Herschel's General Catalogue of Nebulæ, and made immediate search for my mysterious find, of which there was no record.

After my removal to Rochester I received a letter from Professor Barnard then of Nashville, Tenn., saying he had discovered a large nebula not in General Catalogue which he, at first, had mistaken for a comet.

On exceedingly favorable occasion as to purity of the atmosphere, and before the erection of the electric street lights, I examined it with the 16-inch instrument of this Observatory, when I saw it as I have never since been able to do, even under conditions apparently as favorable. An imperfect illustration of its then appearance will be found on page 57, Vol. III of THE SIDEREAL MESSENGER, and on page 21 of "History and Work of the Warner Observatory." Although I have not since seen it as an ellipse with condensations at each focus, yet I have observed what I, at that time, failed to notice; viz., three knots of considerable extent north preceding the nebula, seemingly connected with each other not only, but also with the parent nebula.

Thus the matter rested until Barnard informed me that, with the 12-inch glass of the Lick Observatory, he had discovered a nebulous ring entirely surrounding the cluster which I, myself, also saw at my visit to that observatory, and found that my nebula, as also the three knots, formed part of the ring.

Professor Barnard in *Astronomische Nachrichten*, No. 2918, has given an interesting account of his discovery, accompanied by a faithful illustration of not only the cluster, but also of its inclosing ring, which, the Milky Way excepted, is the largest visible to us, its outer diameter being not less than 40′. In comparison, the ring nebula in Lyra is a pigmy. Mr. Barnard, in his drawing, shows the segment of another ring apparently attached to the principal one on the *s. f.* side, and, judging from the visible portion, it, if continuous, must be nearly as large as the other. This I did not see.

The cluster is H. 2. VII = G. C. 1420, and the nebula is N. G. C. 2237, R.A. 6*h* 24*m* 48*s*, Decl. + 5°8′, in the constellation Monoceros. As it will soon be favorably situated for observation, I shall hope that the distinguished celestial photographer, Mr. Isaac Roberts, will be induced to photograph it, as a change both in brightness and form is suspected.

Warner Observatory, Rochester N. Y. LEWIS SWIFT.
Dec. 20th, 1889.

Large versus Small Telescopes—A Special Case. The following is an attempt to compar the 26-inch equatorial of the Leander McCormick Observatory with the 10-inch of the Haverford College Observatory, by means of the two series of double-star measures made at these observatories. Both

telescopes were made by the Clarks and are probably equal in optical perfection. The micrometer and driving clock of the McCormick instrument are excellent, while the Haverford ones are very indifferent. However the Haverford series of measures have the advantage, in that they were made after the McCormick series. If we assume that, on an average, the atmospheric conditions were the same, we may consider that the two series of measures were made under pretty much the same circumstances.

The observations considered were made by myself, the McCormick series in 1886, the Haverford series in 1888. The probable error of a single night's observation was computed, only those stars being used which were observed at both places. The results are shown in the following table, where *P* represents the probable error in position angle, and *D* represents the probable error in distance.

Mean of Distances.	P	D	Mean of Distances.	P	D	Series.	Magnifying Power.
0″.7	1°.2	0″.07	1″.8	0°.5	0.12	M	850
0 .7	2 .4	0 .06	1 .8	1 .0	0.08	H	375

It was the custom to observe on a given night always the closest and most difficult stars that the definition would allow. The number of difficult stars in a hundred measures will, therefore, furnish another means of comparison. On an average there were 20 measures in a hundred in the McCormick series, where the distance was between 1″.0 and 0″.5, and there were 5.7 measures where the distance was less than 0″.5. In the Haverford series the numbers were respectively 21 and 4.5.

Again, in all the observations with the McCormick instrument the stars were invariably seen as easily separated, while in the Haverford telescope the most difficult ones could not be separated but merely elongated.

F. P. LEAVENWORTH.

Photographing the Corona without an Eclipse. From a brief account, in the December Observatory, of experiments by Dr. Huggins, of England, it seems that astronomers may yet hope to photograph the corona without the aid of an eclipse. This most delicate work needs the best conditions of atmosphere possible, which the climate of England very rarely furnishes. Still Dr. Huggins, after waiting for years for a favorable opportunity, has realized some promise of success. The clear atmosphere of Athens is to be tried in the hope of a speedy solution of this most interesting problem of solar physics. At Captain Darwin's suggestion, Rev. A. J. Perry and Mr. Taylor were to try, during the eclipse of Dec. 21, to learn how long after totality the moon could be seen against the corona. The Lick party obtained a good photograph in this way Jan. 1, 1889.

Chamberlin Observatory. By kindness of Mr. George N. Saegmuller, successors to Messrs. Fauth and Company, Washington, D. C., we have had the privilege of examining the drawings and specifications for the mounting of the new 20-inch refractor for the Chamberlin Observatory at Denver, Colorado. This work seems to combine all the latest and best improvements known to modern astronomy, and the appearance now is that Professor Howe will have the satisfaction of using one of the finest telescopes of which this country or any other can boast. Attention is called to Mr. Saegmuller's new advertisement.

Errata. On page 405, Vol. VIII, line 10 from the top, 1887 should be 1888.

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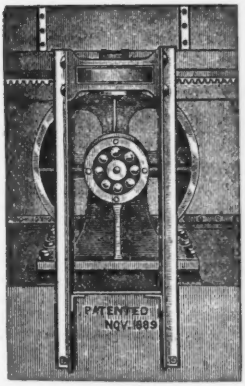

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Term Examinations, March 18th and 19th 1890.

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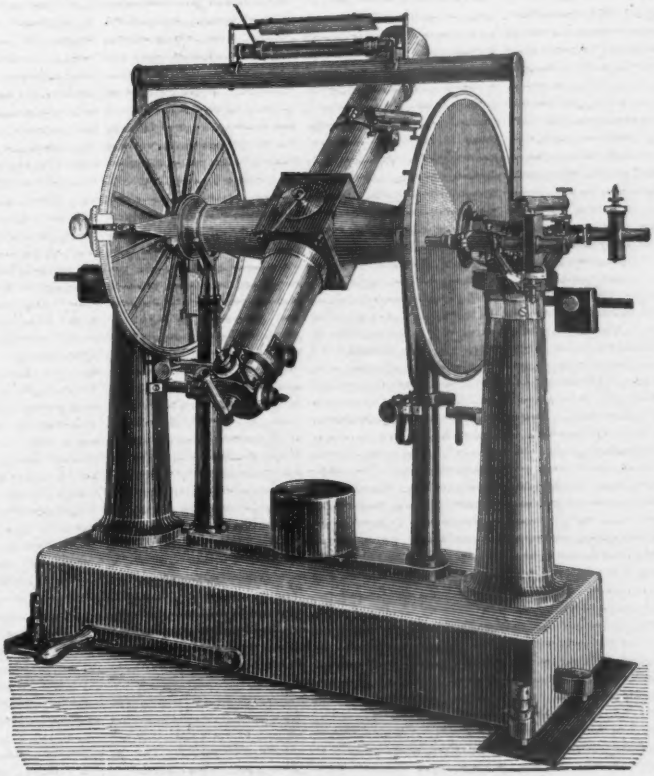

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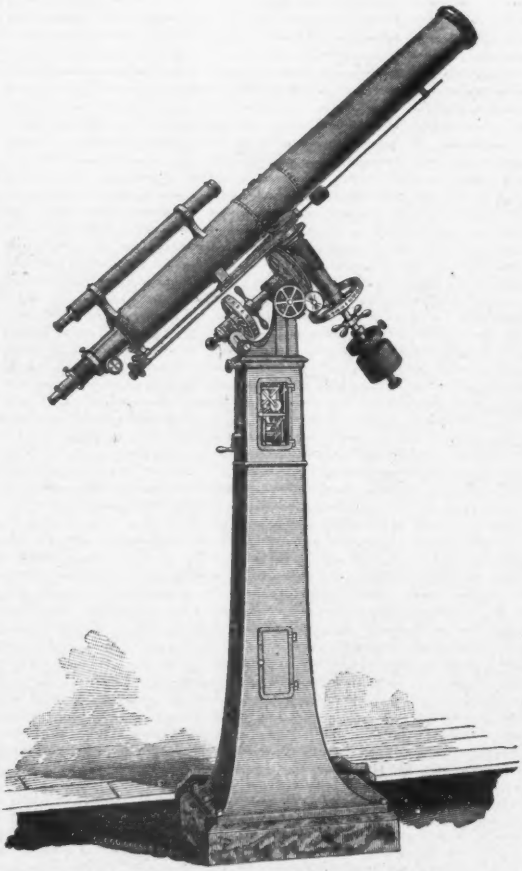

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